Aging and the Labor Market

by

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B.A., Pomona College (2000)

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Abstract

This thesis is a collection of three essays analyzing the interplay between aging and the labor market. The first chapter demonstrates that differential treatment by age exists in labor markets and explores different possible explanations for this differential treatment. As the baby boom cohort reaches retirement age, demographic pressures on public programs such as social security may cause policy makers to cut benefits and encourage work at later ages. This chapter reports on a labor market experiment to determine the hiring conditions for older women in entry-level jobs in Boston, MA and St. Petersburg, FL. I find differential interviewing by age for these jobs. A younger worker is more than 40% more likely to be offered an interview than an older worker. I find no evidence to support taste-based discrimination as a reason for this differential and some evidence to support statistical discrimination.

The second chapter examines more closely one of the possible reason for this differential treatment. Older workers may cost employers more in terms of potential age discrimination lawsuits. I study the effects of state and federal age discrimination laws between 1968 and 1991. Prior to the enforcement of the federal law, state laws had little effect on older workers, suggesting that firms either knew little about these laws or did not see them as a threat. After the enforcement of the federal Age Discrimination in Employment Act (ADEA) in 1979, white male workers over the age of 50 in states with age discrimination laws work fewer weeks per year and are less likely to be hired or separated from their jobs, but are more likely to be retired (perhaps involuntarily). These findings suggest a story in which firms do not wish to hire older workers, are afraid to fire older workers, and remove older workers through strong incentives to retire in states where lawsuits are less of a hurdle for the worker.

The third paper, co-authored with Melissa Boyle, explores the relationship between health insurance coverage and labor market efficiencies termed "job-lock." We exploit an insurance option which is both truly exogenous to work decisions, and of lasting duration. A major expansion in both the services provided and the population covered by the Department of Veterans Affairs health care system allows us to both cleanly estimate the extent of job-lock, and also to study the impact of publicly provided health care on labor supply. Using data from the Current Population Survey, we examine the impact of health care coverage on labor force participation and retirement by comparing veterans and non-veterans before and after the VA expansion. Results indicate that workers are significantly more likely to cease working as a result of becoming eligible for public insurance, and are also more likely to move to part-time work.

Thesis Supervisor: Dora L. Costa
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Contents

Chapter One

Age, Women, and Hiring: An Experimental Study

1.1 Introduction

In a 2004 speech to the Federal Reserve Board, Alan Greenspan suggested that encouraging older people to work could solve many of the problems that will occur as the large baby boom cohort reaches retirement age.[1] If older workers remained in the labor force, social security benefits could be cut without compromising living standards. From a productivity standpoint, workers should be capable of working longer than they had in the past. Not only are people living longer, but several studies suggest that today's 70 year olds are comparable in health and mental function to 65 year olds from 30 years ago (Schaie 1996, Baltes et al 1988). Many older Americans also need to work, even if social security benefits are not cut. Bernheim (1997) estimates that baby boomers on average are only saving a third of what they need to maintain a pre-retirement standard of living after retirement. This lack of adequate retirement savings is especially acute for older widows, who, on average, suffer a 30 percent drop in living standards upon the death of a husband (Holden and Zick 1998). In fact, the poverty rate for older widows is 15% (Favreault et al. 2002). Finally, Abraham and Houseman (2004) find that although most older workers plan to continue working at least part time instead of fully retiring, those who would have to change jobs in order to reduce hours are likely to stop working entirely, suggesting that there is something preventing them from finding a new job.

Will older American women be able to find work? Economists generally assume

[1] Alan Greenspan was not the first to suggest encouraging older workers to remain in the labor force as a partial solution to the Social Security problem, see, for example, Diamond and Orszag (2002).

that staying out of the labor force is a choice for women, so only supply-side factors come into play in policy discussions, such as those regarding social security. This study explores the demand-side of policies that rely on older women finding jobs at the normal age of retirement by running a labor market experiment to explore the hiring behavior of firms seeking entry-level or close to entry-level employees. Although a number of sociology and psychology studies have directly examined age discrimination, these studies typically present a human resources manager (or worse, a group of undergraduate psychology students) with two resumes, one of an older worker and one of a younger worker, and ask which the manager would be more likely to hire (e.g. Nelson 2002). In contrast, this experiment analyzes real rather than hypothetical hiring choices by businesses that do not know they are being studied.

My study examines the entry-level or close to entry-level labor market options for women ages 35 to 62 in Boston, MA and St. Petersburg, FL. I send pairs of resumes to employers in these two cities and measure the response rates by age, as indicated on each resume by date of high school graduation. In addition, I explore reasons for differential responses by age to resumes in several ways. First, I explore statistical discrimination, which is defined as an employer judging a job applicant based on her age-group status rather than on her own individual characteristics. To study this type of discrimination, I look at the effect of resume elements that could signal that the older worker does not fit a stereotype/group characteristic of older workers. Second, I look at employer taste-based discrimination by examining the effect of a firm having a human resources department, since these departments would be likely to have had training in discrimination law. Third, I examine employee taste-based discrimination by looking at the age break-down

of workers in each firm's geographic area. Finally, I examine consumer taste-based discrimination by looking at the residential demographics of each firm's geographic area.

I find evidence of differential hiring by age in these two labor markets. A younger worker is 42% more likely than an older worker to be offered an interview in Massachusetts and 46% more likely to be offered an interview in Florida. Statistical discrimination is the most likely explanation for this differential hiring behavior. This study finds little to no evidence for taste-based discriminatory behavior, whether from employers, co-workers or consumers, although the tests used are not perfect.

Age discrimination is comparatively understudied by economists.[2] Although displaced older workers take longer to find employment than do younger[3], it is not known whether this delay is due to discrimination, higher reservation wages, or clustering in dying industries. Experimental labor market studies such as this one have the advantage of directly observing discrimination as it happens. Such studies have primarily examined discrimination on the basis of gender and race (e.g. Fix and Struyk 1993, Yinger 1998, Neumark et al. 1996). Only one set of these studies (a resume study combined with a matched pairs audit) has explored age discrimination (Bendick et al. 1996, Bendick 1999) and there is concern that this study lacks comparable controls (Riach and Rich 2002).

This paper is structured as follows: Section II gives some background information on discrimination laws, testing for discrimination and types of

[2] When discussing the term "discrimination," I use a value-free definition of the word, such as in Lundberg and Startz (1983) that includes forms of differential behavior such as statistical discrimination, where it is possible for the same average productivity to receive the same average compensation. It does not imply that there is necessarily any animus-based discrimination, simply differential behavior.

[3] The 2000 CPS Displaced Worker Survey finds that the average 12 weeks search time for workers age 55 to 74 was 3.6 weeks longer than that for workers age 19 to 39. Additionally, 39% of displaced older workers in the February 2000 CPS had not found reemployment by the time of the survey, whereas only 19% of those between 40 and 54 had not found reemployment (US General Accounting Office 2001).

discrimination. Section III gives an overview of the experimental design. Section IV describes the empirical framework for both differential hiring by age and reasons for differential hiring by age. Section V provides results and Section VI discusses implications. Section VII concludes. Further information on the specifics of the experimental design can be found in the data appendix at the end of the paper.

1.2 Background

The Age Discrimination in Employment Act, implemented in 1968 and enforced in 1978, covers workers age 40 and up in firms with 20 or more employees, with a few exceptions.[4] This law prohibits discrimination against older workers through hiring, firing, and failure to promote mechanisms. Since it is more difficult for workers to determine why they failed to receive an interview than it is for workers to determine why they have been fired, firms that wish to retain only a certain type of worker without being sued would prefer to discriminate in the hiring stage rather than at any other point of the employment process.

Labor market studies such as this one that test for discrimination in hiring by sending resumes are called "resume audits" in the United States and "correspondence tests" in the United Kingdom. These studies directly test for discrimination with a minimum of omitted variables bias. Other audit studies send two trained "auditors," matched in all respects except the variable of interest, usually race, to rent an apartment, buy a house or interview for a job. In practice, however, it is difficult to match people exactly[5]; one cannot rule out systematic differences observable to the employer between

[4] Firms are exempt if they can prove a bona-fide occupational requirement (BFOQ) that is directly related to age (for example, an acting position), or if the position is a high salaried policy making position.
[5] Other problems with this method are elucidated by Heckman and Siegelman(1993) and Heckman (1998).

the two groups being studied. Experiments such as this one, using randomized resumes, potentially bypass the matching problem. This type of experiment also has the benefit of being able to explore the different reasons that employers might discriminate against older workers.

Economic theory generally distinguishes between two major types of discrimination: statistical discrimination and taste-based discrimination. Statistical discrimination occurs when an individual is judged based on group characteristics. This form of discrimination is generally thought to be efficient for employers in cases of imperfect information (Arrow 1972). For example, if, in general, it is true that older workers take longer to learn unfamiliar tasks, then an employer may be reluctant to hire an older worker, because testing each older applicant for ability to learn is costly. Taste-based discrimination occurs when an employer, a set of employees, or a customer base, gets disutility from working with individuals from a specific group. This form of discrimination is generally thought to be inefficient from an overall welfare point of view, although it provides utility to the discriminator (Becker 1971).

1.3 Experimental Design

I sent resumes to 3996 firms in the greater Boston, Massachusetts and greater St. Petersburg, Florida areas. Boston was chosen for convenience and St. Petersburg was chosen because it both has a similar demographic mix to what the US Census projects the entire United States to have in the 2020s, that is, it has a large concentration of elderly, and because the St. Petersburg/Tampa Bay MSA is approximately equal in magnitude to

the Boston PMSA.[6] Each Sunday, 40 want-ads were culled from the Sunday Boston Globe and 40 from the online version of the Sunday St. Petersburg Times.[7] Monday through Wednesday of each week, company names and numbers were randomly selected from the Verizon superpages for Boston and for St. Petersburg and 10 firms were chosen in each city as "call-ins." A computer program mixed and matched work histories and other resume parts from actual entry-level applicants to randomly create new resumes for specified positions. Genuine resumes for many different job categories were taken off of online sites such as www.americasjobbank.com. These resumes were sorted by occupation and age and had items such as previous work experience, licensure, awards, hobbies and volunteer work collected together and entered into a computer program. Summary statistics for the resumes can be found in the Appendix Tables 1a and 1b . Employers could reply to the job seekers via a voicemail box obtained from www.mynycoffice.com and an email address from www.hotmail.com. Detailed information on the process of resume creation and distribution can be found in the Data Appendix.

For the most part, the resumes created for the audit used items from actual resumes (but not in any way that could be connected to the original resume). Two items included in some of the sent resumes did not appear in actual resumes: the specific places of high school graduation and a declaration of health insurance status. Two schools from small college towns from the Midwest were chosen so that employers could not use perceived high school quality (from 17 to 44 years ago) as a signal for worker

[6] The Boston PMSA had a population of 3,285,387 in 1998, Tampa/St. Petersburg had a population of 2,254,405 in 1998.
[7] The St. Petersburg Times puts all of its want-ads online, whereas the Boston Globe charges employers extra to be included in the online listings.

quality. Some resumes in the experiment included a statement that the applicant did not need health insurance benefits. First names chosen for the job candidates were the first and second most popular female names in the United States for the year of birth of that candidate (Mary and Linda), and the last names chosen were the first and second most popular last names in the US (Smith and Jones), according to social security administration data.[8] The addresses chosen were from middle class neighborhoods which, according to the census, had a wide variation in income and other demographic characteristics (for example, Somerville, MA).

Unlike race or gender, age is a continuous variable. Because I use multiple ages in my study instead of only two ages (e.g. as in Bendick et al. 1999), I can better understand how age interacts with hiring decisions. I chose age 62, the early retirement age, rather than 65, the full retirement age, as the later endpoint for the survey because 65 is the age Medicare benefits generally begin and thus could signal lower health care costs to potential employers. I did not use ages earlier than 35 for two reasons. First, I had to limit the total number of ages I used in order to be able to collect a sample size large enough to preserve power and since my focus is on the older ages, 35 seemed more natural than, for example, 25, as a cutoff.[9] Second, because the current resume standard is to display a 10 year job history, I wanted a potential employer to assume that each applicant was doing the same thing during that ten year history if there were gaps in the resume (e.g. taking care of her family and not going to school).

Word of mouth, not formal advertisement, accounts for most job matches, according to Holzer (1996). However, formal methods are still important, especially for

[8] http://www.ssa.gov/OACT/NOTES/note139/original_note139.html
[9] Initially I had chosen 45 as my "start" age, but after I obtained additional funding, I was able to add 35 before the start of the actual study.

those lacking social networks. To get a more representative sample of job openings than can be found through the want-ads alone, I added 10 entries per city per week generated from calling companies randomly chosen from the Verizon yellow pages.[10] The response rate for call-ins was about half that for want-ads. However, the ratio of older positive/interview responses to younger was very similar whether the ad had been generated via want-ad or via call-in[11], thus providing some evidence that the degree of differential hiring does not vary much with method used, at least if the method still has some degree of formality. For more information on how these "call-ins" were generated, see the Data Appendix.[12]

Resumes were sent in pairs via fax.[13] A coin was flipped each time a pair of resumes was sent to determine which would be sent first. Via the randomness of the computer program used to create resumes, employer bias was randomized across each high school graduation date. Employers who left at least two messages for the prospective candidate were informed in a timely fashion that the candidate had already accepted a job elsewhere so as not to inconvenience area firms. Overall I had a "positive" response rate of 8% in Massachusetts and 10% in Florida and an "interview" rate of 4% in Massachusetts and 5% in Florida.

To distinguish between age discrimination and discrimination based on

[10] I did not add more because the marginal additional call-in was much more time intensive to collect than the marginal additional want-ad.

[11] The exact ratios of younger positive responses to older (keeping in mind older contains more dates of high school graduation than younger) for Massachusetts are: .778 positive if want-ad, .770 if call-in; .92 interview if want-ad, 1.00 if call-in. For Florida these ratios are: Positive: .763 if want-ad, .741 if call-in; Interview: .906 if want-ad and 1.14 if call-in. The ratios of negative/null responses follow a similar pattern.

[12] Online resume clearinghouses were also tried, but, since the economy had cooled by the time the experiment started, the responses they generated were representative of what one finds in one's spam filter.

[13] Only two resumes were sent to each employer because an employer would be likely to get suspicious should he or she receive four virtually identical resumes in a short time period, whereas two resumes are much more likely to be thought of as a coincidence if noticed at all.

differences in human capital or based on perceived gaps in work history, I employed a number of design measures. First, I only sent resumes for women because an employer is more likely to assume that a woman entering or re-entering the labor market has been taking care of her family, rather than returning from prison or a long spell of unemployment, as would be the case for a man. Second, I limited work histories to 10 years, since conversations with human resource professionals and an examination of actual resumes suggested that this length is common practice.[14] Third, I indicated that the applicant was currently employed at an entry-level job so that all applicants had current experience at some form of work (thus diminishing fears that older workers had a longer time for human capital to deteriorate). Finally, I limited my study to entry-level jobs, where entry-level is defined as anything which requires at most one year of education plus experience combined. For these jobs, job-specific human capital should be less of a concern.

Although these limitations can only say something about a specific segment of the labor market, my controls are comparable enough that my results can be trusted. Additionally, I study an important segment of the labor force; the population of older women is larger than that of men and older women are more likely to be living in poverty than men (Favreault et al. 2002). Finally, many entry-level or close to entry-level jobs,

[14] I spoke to human resource professionals from three places—first, several professionals from the hiring department from a large university, second, someone who had worked as a human resources professional for a business firm, but had recently had a career transition to a post where she helped other people determine career transitions, and third, two representatives from a non-profit temporary agency/career placement firm. They all said that ten-year histories are the current gold standard for resumes, although they get many resumes that do not look like the standard. The placement agency said that a big part of their job was to get applicants to make their resumes look like the current standard and the university hiring department said that using an outdated resume style was often an indication that the applicant was older. The university hr department told me that while one was not supposed to put dates of education on resumes, most people did, and it was generally in an applicant's best interest to put down dates of education if it was recent.

such as cashier positions, secretarial work, or home health care tend to be female-dominated jobs, and thus it would not seem unusual for a woman to apply for these positions, whereas a man applying to these positions might be considered suspect.

1.4 Empirical Framework

1.4.1 Differential Hiring by Age

To test for differential hiring by age, I sent paired resumes matched on all characteristics except age[15], as indicated by date of high school graduation, to prospective employers in the entry-level labor market. Then I measured the rate of positive responses and interview responses by age. Positive or "callback" responses are those where the applicant was called back and given a "positive" sounding response but not specifically offered an interview. Examples included asking the applicant to call back or saying that the caller has questions. They did not include responses that are obviously negative, such as information that the position has been filled. Interview responses specifically asked the applicant to call back to set up an interview or to meet in person.

There are many ways to measure age given my setup. I looked at high school graduation cohort dummies, age as a continuous variable, and a dummy which broke high school graduation cohorts into two groups: older and younger. The results should be similar, but different age configurations give varying amounts of power.

First I ran differential probits on positive response and interview responses using high school graduation dummies as the independent variables of interest:

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[15] It is of note that I did not need to match the resumes on characteristics since I use standard differential probit methods to analyze the data rather than the audit methodology of a "paired difference of means" test. Since I targeted a large number of firms, and the resumes were sent randomly, I should get the same results with the regressions I run even had I not matched the resumes. Indeed, since there are five possible ages, it is not even clear what the proper "paired difference of means" test should be.

$$\text{pr}[Response_i = 1] = \text{pr}[B_1(Controls)_i + B_2(Graduation\ Cohort)_i + \varepsilon_i] \quad (1)$$

$$= \Phi[B_1(Controls)_i + B_2(Graduation\ Cohort)_i]$$

where Φ represents the normal CDF. The tables report the marginal effects, ∂ prob $(Response_i = 1)/ \partial X_i$, where X_i is the vector of explanatory variables. Here, *Response* is either a positive response or an interview response, *i* refers to the individual, and the set of graduation cohort dummies include indicators for graduating in 1959, 1966, 1971, 1976 and 1986. *Controls* include the number of years of work history out of 10, typos, college experience, relevant computer experience, volunteer work, sport, other hobby, insurance, flexibility, attendance award, and a set of occupation dummies. Since the explanatory variables are dummy variables, this differential probit reports the discrete change in the probability of interview for each variable.

A second way to test for discrimination is to treat age indicated on resume as a continuous variable using a probit:

$$\text{pr}[Response_i = 1] = \Phi[B_1(Controls)_i + B_2(Age)_i] \quad (2)$$

Then the marginal effect ∂ prob $(Response_i = 1)/ \partial X_i$ represents the discrete change in the probability of a positive response or interview for each of controls, and the infinitesimal change in the probability of interview for age. *Controls* are as reported before.

Finally, employers may mentally group workers into "older" and "younger" categories. I break up high school graduation dates into two groups, one for workers age

50 and older and one for workers under age 50, and run a t-test comparing the probability of being called back or interviewed for each group. To compare older and younger groups, controlling for resume and industry characteristics, I run an OLS regression for each group:

$$Response_i = B_1(Controls)_i + B_2(Older)_i + \varepsilon_t \qquad (3)$$

and retrieve the predicted probability for the response. Then I run a t-test comparing these predicted probabilities for each group. Alternatively, another test for these two groups, again using the probit, is:

$$\text{pr}[Response_i = 1] = \Phi[B_1(Controls)_i + B_2(Older)_i] \qquad (4)$$

Where *Older* is a dummy indicating that the worker is age 50 or older. The marginal effect $\partial \text{pr}(Response_i = 1)/ \partial X_i$ represents the discrete change in the probability of interview for each variable.

1.4.2 Reasons for Differential Hiring by Age

1.4.2.1 Statistical Discrimination

My experimental setup enables me to explore different possible reasons for this differential hiring, or discrimination.[16] The first type of discrimination I look at is

[16] I do not differentiate between stereotypes which are true (and thus fit in standard models of statistical discrimination, such as Phelps (1972)) and stereotypes which are false, but employers believe to be true. One can make the argument that since workers who are hired young often age into the firm, that firms which employ larger numbers of workers may have some experience with older workers and are less likely

statistical discrimination, which in its most basic definition, is judging an individual on group characteristics rather than individual characteristics. More formally, I assume the model by Phelps (1972) as outlined in Aigner and Cain (1977) where I assume that employers measure expected skill through an indicator y based on the observed true skill level q and a measurement error u, thus $y = q + u$. I assume that the variance of u is equal for the two groups and the variance of q is greater for older workers than for younger.[17] This model provides a framework where positive information about the ability, that is a higher y, helps older workers more than it helps younger workers (the y-E(q) graph will have a steeper slope for older workers than for younger). For example, an indication that an older worker has taken a computer class will cause a greater marginal increment to expected productivity for the older than younger worker, that is, it will help an older worker more than it will help a younger worker.[18]

I tested for statistical discrimination by randomly including items on resumes that signaled that the worker did not fit into a standard stereotype.[19] For example, to test whether employers think older workers are inflexible and unchanging, I include a statement that the applicant was flexible or "willing to embrace change." To test for the effect of these variables on the probability of getting a callback or interview, I interact

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to believe false stereotypes. Additionally, the notion of feedback effects (as in Lundberg and Startz 1983) into educational choices is less of an issue because even though older workers may choose training, the majority of education decisions have already been made. There may still be feedback effects in terms of decisions whether or not to remain in the labor market, however.

[17] Average true ability for the two groups is assumed either equal or that true ability is lower for older workers than for younger.

[18] Different assumptions provide a model where the test is less reliable for older workers and thus a positive ability signal would help younger workers more than older. However, there is no reason to assume that either younger workers have larger variance in, for example, computer ability or would get more out of a basic computer skills course than older workers, unlike the case where many black high schools are assumed to be of more variable/worse quality than many white high schools.

[19] Stereotypes examined came from a list of the top 10 reasons for discrimination against older workers according to a 1984 survey of 363 companies where hiring managers were asked for reasons that other companies might discriminate against older workers (Rhine 1984). Not all top 10 reasons could be explored using this experimental design.

each of these variables with *older:*

$$\text{pr}[Response_i = 1] = \Phi[B_1(Controls)_i + B_2(SDR_i) + B_3(Older_i) + B_4(Older_i * SDR_i)] \quad (5)$$

Where *SDR* is the reason for statistical discrimination which is being tested and *Controls* include the number of years of work history out of 10, typos, college experience, relevant computer experience, volunteer work, sport, other hobby, insurance, flexibility, attendance award, and a set of occupation dummies, except when the reason tested is one of those controls.

Since an interaction term is measuring the difference between the slopes of the *SDR* term when *Older* = 0 and when *Older* = 1, I can measure the same results by running the regressions separately for each group. I also run regressions on just the controls and variables of interest (not including an age-related variable) separately for older and younger groups and compare coefficients. This format is identical to the interaction:

$$\text{pr}[Response_i = 1] = \Phi[B_1(Controls)_i + B_2(SDR_i) + B_3(Older_i) + B_4(Older_i * SDR_i)] \quad (6)$$

Where *SDR* here is a vector of reasons for statistical discrimination and other variables are as described earlier. Showing regressions separately for *Older* = 1 and *Older* = 0 has the benefit of efficiently showing multiple interactions at the same time.

Another method to differentiate between statistical discrimination and employer taste-based discrimination using the presence of a human resources department is

described in the next section.

1.4.2.2 Taste-Based Discrimination

Employer

Human resource professionals may have less taste-based discrimination because of training and knowledge of discrimination laws, although they might be more likely to practice statistical discrimination through learning.[20] Thus, I study employer discrimination by interacting a dummy indicating whether or not a company has a human resources department, *HR,* with age:

$$\text{pr}[Response_i = 1] = \Phi[B_1(Controls)_i + B_2(HR_i) + B_3(Older_i) + B_4(Older_i * HR_i)] \quad (7)$$

where *HR* is a dummy variable indicating whether or not a company has a human resources department. *Older* and *Controls* are as described before.

Employee

My tests for employee taste-based discrimination and customer taste-based discrimination rely on the assumption that people are less likely to discriminate against those in their own group. To study employee discrimination, I interact age with *Over*50*Work,* the percentage of people employed in the area where the business is located over the age of 50:

$$\text{pr}[Response_i = 1]$$

[20] Unlike the usual case for race or gender, one's age status does change while employed. Thus an employer can end up observing the productivity of a group of older workers even if it never hired older workers.

$$= \Phi[B_1(Controls)_i + B_2(Over50Work_i) + B_3(Older_i) + B_4(Older_i * Over50Work_i)] \quad (8)$$

Here *Over*50*Work* is a continuous variable indicating the percentage of people over the age of 50 who work in the firm's place of work PUMA, as indicated by the 2000 census. *Controls* and *Older* are defined as above.

Customer

My test for customer taste-based discrimination is similar to that of employee taste-based discrimination, except that instead of looking at the percentage of people employed in an area, I look at the percentage of people who reside in the area where the business is located:

$$\text{pr}[Response_i = 1]$$

$$= \Phi[B_1(Controls)_i + B_2(Over50_i) + B_3(Older_i) + B_4(Older_i * Over50_i)] \quad (9)$$

Here *Over*50 is a continuous variable indicating the percentage of people over the age of 50 who live in the firm's zipcode, as indicated by the 2000 census. Since people who work in services and sales are more likely to interact with consumers, I also run equation *(9)* using only service and sales occupations.

1.5 Results

1.5.1 Differential Hiring by Age

Figures 1a and 1b show an upward trend for the positive response based on date

of high school graduation, as in equation (*1*). This trend is much more marked using interview as the dependent variable. Although no two adjacent years are statistically significantly different from the 0 to 5 percent level, the results are suggestive. In Massachusetts, the interview results show a statistically significant difference at the 5 percent level between the oldest, *hsgrad59*, and youngest *hsgrad86*. Interview results may be stronger than positive for two reasons. First, not all "positive" responses may actually be positive—some asking for more information could be preludes to rejection, thus producing measurement error. Secondly, more subtle forms of discrimination, such as calling one person back more enthusiastically than another, are less likely to be discovered than overtly failing to call back the older candidate. In fact, the caller may not even realize that he or she has treated the candidates differently.

The most significant results are found breaking up age categories into "older/younger" groups where older is defined to be age 50, 55, and 62 and younger is defined to be ages 35 and 45.[21] Table 1 describes t-test results comparing the mean response rates for these two age categories with controls as calculated in (*3*) and without controls. For callbacks, there is a difference of 1.5 percentage points, or 19%, in Massachusetts and 1.7 percentage points, or 18%, in Florida. For interviews, these differences are 1.6 percentage points, or 42%, for Massachusetts and 2.0 percentage points, or 46%, for Florida.[22] The average younger job seeker in Massachusetts needs to file, on average, 11 ads to get one callback whereas an older needs to file 13. A younger

[21] I also tried breaking up older and younger categories by placing 50 in the younger category (older2 and younger2) and leaving 50 out altogether (older3 and younger3). Results were similar across categories but, defining 50 as older produced the strongest results.

[22] If I take the lowest point in the confidence interval for younger workers and divide that by the highest point in the confidence interval for older workers, and then do the same with the highest point for younger workers and lowest point for older workers, I get a range of a younger worker being -.05 to 113% more likely to get an interview in Massachusetts and -.02 to 117% more likely to get an interview in Florida.

seeker needs to file 19 ads for one interview request and an older job seeker 27. In Florida, a younger worker needs to file 9 and an older worker 11 ads to get a positive response. These numbers are 16 and 23 respectively for an interview response. A probit including *older* as an age dummy, as in equation (*4*), results in a negative and significant coefficient for *older* for interviews in Massachusetts and Florida and callbacks in Massachusetts, as shown in Table 2.

A final way of looking at the effect on age is to actually regress on age as if it were a continuous variable as in equation (*2*). This method provides more power than using age dummies. Table 2 shows that the marginal effect coefficient on age is negative but not significant at the 5% level such that for each additional year of age, a worker is .07% less likely to be called back in Massachusetts and between .04% and .06% less likely to be called back in Florida.[23] This effect is both negative and significant at the 5% level for the interview response, with each additional year of age causing a worker to be .07% less likely to be called back for an interview in Massachusetts and between .067 and -0.09 less likely to be called for an interview in Florida. Thus there is differential interviewing by age. Specifically, assuming linearity[24], in Massachusetts, the mean applicant would have to answer 1 additional ad to receive a callback for each additional 10 years of age, and 4.5 additional ads to receive an interview request. In Florida, each additional 10 years of age would require 5 more ads to produce a callback and 3.5 more ads to produce an interview (interview responses have a lower base rate, so even though

[23] Depending on whether or not controls are included. Since employers may treat certain characteristics differently depending on age, in a non-linear probit model the coefficient of age can change based on whether or not they are included, even if the characteristics are randomized across resumes. In ordinary least squares model the coefficient would not change. Additionally, although age is uncorrelated with the controls by design, in a finite sample there may still be correlation induced by chance.

[24] An age squared term came up insignificant in probit regressions. However, I cannot reject a cubic age specification for the interview response in the Florida set. The cubic age specification is not significant in the Massachusetts set.

the percentage decrease by age is more interviews, .067% rather than .04%, this decrease translates into a worker having to apply to 10.7 rather than 10.2 ads for each year of age to get a callback and 23.2 rather than 22.9 ads for each year for an interview).

Companies could also discriminate in more subtle ways than failing to call back or to ask for an interview. Other possible outcomes are calling back the younger applicant sooner than the older applicant, or calling back the younger applicant multiple times but only calling the older applicant once. Although there are examples where either of these outcomes is the case, on average there is not statistically significant discrimination for either of these possibilities (results not shown). I also briefly looked at actual negative responses, but not only were there very few of these, but I have reason to believe that when negative responses are sent out, many of them are sent via postal mail.[25] Since I do not have information on postal responses for the majority of applications, it is not feasible to use negative responses as an outcome.

1.5.2 Reasons for Differential Hiring by Age

Economists recognize two main categories of discrimination: statistical discrimination and taste-based discrimination. Statistical discrimination can occur based on observables, such as work history or typographical errors, or unobservables, such as energy or ability to learn. In my experimental setup, observables are identical for each resume pair sent and thus cannot be responsible for the differential hiring. To look at the effect of unobservables, I included items on the resumes to signal that the applicant did

[25] In the Massachusetts part of the sample, I was able to collect mail at one of the two addresses that were randomly assigned to resumes. Through this collection, I did not find any positive or interview responses, but did receive some negative responses. The majority of written responses were post-cards stating receipt of the application. There were a few requesting more information, but these also requested more information via phone or email as well.

not fit a number of stereotypes cited by managers as reasons firms might be reluctant to hire older workers (Rhine 1984). The effects of these variables are discussed in more detail below and are detailed in Table 3 which gives results from running equation (*5*) separately by older status.

1.5.2.1 Statistical Discrimination

Employers may statistically discriminate because they fear that older workers will "cost" more in terms of absences and benefits. To test whether or not companies statistically discriminate against older workers because they assume older workers will have more absences, I introduced an item on the resume saying that the applicant has won an attendance award. This variable is positive but not significant at the 5% level. If anything, attendance awards help younger workers more than older in terms of magnitude. To see whether or not higher health insurance costs are a reason older workers are not hired, I put in the statement that a worker does not need insurance coverage.[26] Although having insurance seems to help getting a callback overall in Massachusetts, nothing can be said by age at the 5% level. Already having insurance increases the likelihood of getting a callback or interview in Massachusetts, but helps only younger workers and may hurt older workers in Florida, although, again, these results are not significant. Employers could also fear that older workers may be less likely to have reliable transportation, and thus may be tardy or absent from work for this reason. There is no evidence that commute time, matched by zipcode to place of work

[26] Although, according to Blue Cross/Blue Shield (personal communication), health insurance costs generally stay the same for women until the age of 65 (the possibility of pregnancy goes down as a woman ages) there is some doubt that human resource managers are aware of the fact. Scott et al. (1995) find that older age hiring is lower in firms that offer health insurance. However, firms that offer benefits such as health insurance are different than firms which do not. For example, they tend to be larger and have steeper earnings profiles as well (Idson and Oi 1999).

PUMA affects older or younger workers differently (results not shown).

Employers may also worry that older workers will not be as productive as younger. First, they may believe that older workers' knowledge and skills are obsolete. For this reason I added a variable indicating that the worker had gotten a computer certificate in 1986 (which would be outdated), 1996 or 2002/2003 when such skills would be relevant and recent. Although not significant, relevant computer experience helps younger workers to get interviews in Florida more than older workers. However, in Massachusetts, it helps older workers more than younger, although the interaction term is only significant at the 40% level.[27] Vocational training[28] helps younger workers more than older workers to get both callbacks and interviews. An interaction between vocational training and older (not shown) gives this result to be significant at the 5% level for Florida, but not for Massachusetts. Second, employers may be worried that older workers lack energy. To test this reason, I introduced an item on the resume saying the applicant plays sports. For the most part, this variable is not significant. It is significant and negative for the callback response for younger workers in Massachusetts and significant and positive for the interview response for younger workers in Florida. Although an interaction term shows putting sports on the resume to hurt older workers less than younger workers, this finding is only significant for positive responses at the 20% level in Massachusetts.

Third, previous research has suggested that older women use volunteer work as a "stepping stone" to labor market work (Stephen 1991), and, indeed, I find that having

[27] Interaction results have also been done using the Norton adjustment, and results still hold (Norton et al. 2002). Magnitudes may change, but signs and 5% significance do not.
[28] Note that occupation and vocational training are mechanically related in this experiment because vocational training was only given to resumes for which it was required (such as dental assisting or nursing).

volunteer work listed helps older women more than younger.[29] Fourth, Bendick (1996) found that the biggest help to an older worker's resume was to signal that he or she was flexible or "willing to embrace change." Although only significant at the 10% level for Massachusetts, I found that having this statement on a resume hurts an older worker, but does not hurt a younger worker.[30] This difference in findings may be because the AARP has been recommending that older workers put such statements on their resumes since the time of Bendick's study and thus this statement now signals that the worker is old.

Finally, experience may interact with age as a form of statistical discrimination. Employers may assume that older workers have more experience, or they may be prejudiced against an older worker if she does not have more experience than a younger worker. I looked at this issue in two different ways. First, I looked to see what effect having experience in the same occupation for which the worker was applying had for the different age groups. Although no interactions of same experience with age are significant at the 5% level (not shown), having occupational experience listed on the resume similar to occupation being applied to the helps younger workers more than older workers as shown in Table 4. However, a different effect is found for implied experience—that is, when the want-ad requires experience[31]; older workers were hurt less than younger workers, as shown in Tables 4, although again, this finding is not significant at the 5% level. Thus there is slight evidence that employers are more likely to give older workers the benefit of the doubt in terms of experience, but only when

[29] The interaction of older and volunteer is positive and significant at the 25% for positive outcomes and 30% level for interview outcomes in Florida, but only at the 46% level for interview in Massachusetts.
[30] The interaction of older and flexible is significant at the 14% level for the interview variable in Massachusetts and at the 70% level for Florida.
[31] Admissible want-ads could include requirements of up to a year of experience, whether the applicant had it on the resume or not.

neither applicant lists the required experience on the resume. Otherwise, having the required experience may help younger workers more than older. This possibility suggests that the entry-level labor market may be different in terms of age discrimination from markets requiring more experience.

1.5.2.2 Taste-Based Discrimination

Employer

One form of taste-based discrimination is employer discrimination, in which the employer him or herself (or those doing the hiring) prefers one group over another based on his or her own tastes. Human resource professionals may have less taste-based discrimination because of training and knowledge of discrimination laws, although they might be more likely to practice statistical discrimination based on learning from past hires. Bendick (1994) assumes that firm size is a proxy for having a human resources department and finds that there is no link between race discrimination and firm size. I found no link between having a human resources department and being more or less discriminatory using equation (7). In my study, firms with human resources departments may be more likely to interview younger workers, which would support the case of statistical rather than taste-based discrimination, but this finding is not significant.[32] The controlled coefficient on the interaction term between *Older* and *HR* for Florida for the interview outcome is -0.007with a standard error of 0.018 and this coefficient for

[32] Another possible way of measuring employer taste-based discrimination is to examine the hiring interaction between the ages of employers or human resources professionals and applicants. However, I have been unable to collect information on employer age. Additionally, just because an employer is a member of a group does not mean that he or she will not discriminate against other members. For example, Dick Clark, age 76, was recently sued for age discrimination *http://www.cnn.com/2004/LAW/03/02/dick.clark.sued.ap/.*

Massachusetts is -0.017 with a standard error of 0.0195.[33]

Employee

Another form of taste-based discrimination occurs when employees prefer to work with members of a certain group. Younger employees might prefer to not to work with older employees, especially when the older employee is in a subordinate position. To test for this type of discrimination, I match zipcodes from my dataset to place of work PUMA information on worker age from the census and look at the effect of percentage of workers over 40, over 50 and over 61 employed in the PUMA.[34] I found no effect of the age of a company's workforce on the differential hiring by age, thus providing no support for employee taste-based discrimination (results not shown).[35] However, this measure may be too crude, as it matches zipcode to place of work PUMA information rather than using the percentage of workers by age in a firm.

Customer

A final source of taste-based discrimination comes from the consumer base. Consumers may prefer to buy from or interact with employees who are like them. To test for this type of discrimination, I used the census to get age profiles of zipcodes in Florida and Massachusetts and matched them to the zipcodes of the companies applied to in the study. Taste-based discrimination should be even higher in occupations where there is interaction with the public, such as in sales and service. There is no evidence of

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[33] A Norton correction does not change the sign or significance of these effects. The main effects for Florida are -0.008 for older and 0.010 for HR. In Massachusetts, these effects are -0.021 for older and 0.018 for HR.

[34] This effect of older workers in a company influencing the age of new hires is not mechanical because older employees may have been hired young and aged with the company.

[35] For the percentage over age 50 interaction with older, the FL coefficient is .00139 with a standard error of .00286 and the MA coefficient is -.660 with a standard error of .478.

consumer taste-based discrimination; areas with higher percentages over the age of 50 are more likely to call back or to interview in general and these results are stronger for younger workers than for older. The results are similar when only service and sales positions are looked at (results not shown). Thus there is no evidence that younger consumer bases prefer workers in the same age group.

1.6 Implications

These differential responses have real implications for older potential workers. One may wonder, "So older workers have to send in a few more applications to get an interview, so what?" Aside from the psychological implications of implied rejection, there are economic consequences to this sort of differential that are more severe for some occupations than others. First, the number of applications sent to receive an interview vary by occupation. Using general occupation categories, the number of applications needed for an interview ranges from a low of 5.5 for younger workers and 10 for older workers in healthcare positions in Florida to a high of 32 ads for younger workers and 72 ads for older workers seeking clerical positions in Massachusetts.[36] Second, many occupations have a limited number of positions advertised each week. For example, on a randomly chosen Sunday in Florida, there were 34 LPN jobs being offered but only 8 pre-school teacher positions. For some professions, such as jewelry appraiser (which requires 6 months to a year of training), it is possible to call almost every jewelry store in

[36] With "low" and "high," I am only including general occupation categories that have at least 200 resumes sent. There are some occupational categories with low sample sizes, such as professional/technical non-healthcare (mostly pre-school teachers) in Florida that received no responses for older workers, and thus would, by the metric used, require an infinite number of resumes to receive an interview. However, only 51 resumes were sent to p/t non-healthcare positions in Florida. There were 558 healthcare resumes sent in Florida and 1057 clerical resumes sent in Massachusetts.

the area over the course of a year and only net one interview.[37] Finally, given that the wages for many of these occupations are not very high (often minimum wage), it is likely that persons seeking these jobs also do not have a large amount of wealth to finance an extended job search, especially if they cannot receive employment benefits.

What does this mean for older vs. younger workers? Conditional on getting an interview response, it takes on average 8 days to be offered an interview. I have not been able to find information on the number of interviews it takes to get an entry-level job, but one online firm[38] finds that it takes 7-10 interviews on average for a college graduate to obtain a job offer. Using a back of the envelope calculation with one of the professions most likely to be hired, a new licensed practical nurse[39] sending out ~30 applications a week can expect 3 interviews a week as an older worker and 6 interviews a week as a younger worker. Assuming it takes 7-10 interviews to land a job, a younger worker could expect an employment offer in a little over a week, and an older worker 3 weeks. But this is the best case scenario. An older worker attempting to find clerical work could file close to 100 applications per week and expect to be given an offer 7 to 10 weeks later (a younger worker would get an offer in half that time), using the same back of the envelope calculation, and that is only assuming that there are 100 unique new clerical ads each week, which, since a large number of ads are run at least two weeks in a row, is unlikely. For someone who needs to work because of a lack of savings, several months without income could be critical.

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[37] Which was the case in Florida.
[38] www.onestop.com
[39] A profession which takes 1 year of training and had a median salary of $31,440 in 2002 according to the BLS *Occupational Outlook Handbook.* http://bls.gov/oco/ocos102.htm

1.7 Concluding Comments

This study clearly shows differential interviewing by age for entry-level positions in contemporary labor markets. I found that younger applicants are 44% more likely to be requested for an interview in Massachusetts and 43% more likely in Florida. The extent of discrimination against older workers is similar to that of discrimination against women or blacks.[40] I found no evidence of taste-based discrimination. I found some evidence for statistical discrimination against workers along a few dimensions, such as skills obsolescence, as signaled by adding relevant computer experience to a resume (but only in Massachusetts). Many resume items helped younger workers but either hurt or did not affect older workers.

Future research needs to be done both exploring other labor markets, such as the non-entry-level market, and pinpointing additional reasons for statistical discrimination. In non-entry-level positions, there may be taste-based discrimination against younger workers supervising older workers, which would suggest that there would be less age discrimination against older workers in these markets. For example, managerial positions in Florida (but not Massachusetts) tended to prefer older workers, interviewing 4% of older applicants and 1% of younger workers. I also found differences in differential hiring between occupations; Blue-collar and male-dominated occupations in the sample tend to prefer older workers to younger. Since these occupations in my sample tend to be clustered in dying industries, there may be a bias towards hiring workers with shorter

[40] Neumark et. al (1996) find evidence of 47% differential interview requests against female waitstaff in high-price restaurants and 40% towards female waitstaff in lower-price restaurants. Bertrand and Mullainathan (2003) find that applicants with white sounding names are 50 percent more likely to be called for an interview than applicants with black sounding names. It is somewhat difficult to compare the extent of the magnitude of age discrimination to race or gender discrimination, since age is not a binary variable and breaking into older and younger categories can be done arbitrarily. I might have found more had I been comparing, for example, 32 year olds to 90 year olds only.

expected future work-lives.

Another reason for discrimination against older workers that could not be tested in this set-up is that older workers cost more because they can sue employers under the Age Discrimination in Employment Act. In this volume, Lahey (2005b), looks at the effect of age discrimination laws on older workers. Although it finds that these laws have a significant and negative effect on older white men, it does not find a similar effect for older women. Since the current cohort of older women is unlikely to sue, employers may not take possible litigation into consideration in the hiring process.

This study provides evidence that the demand for labor from older workers is smaller than that for younger workers. Simply encouraging older workers to reenter the labor force will not guarantee that they will be able to find jobs in a timely manner, if at all. This study also has important implications for women who are most likely to need additional work—those with little work experience who unexpectedly need to enter the labor market, such as widows, those whose husbands have lost jobs and cannot find employment, or divorcees. Although there are more older women than older men, the majority of economic surveys on aging and work focus on a random sample of men and, if they include women at all, only include spouses. Any policy which depends on older people finding work to maintain their quality of living, such as changing social security benefits, needs to consider this demand side.

1.8 Data Appendix

The use of a computer program to randomly generate items to create many different possible resumes is a large improvement over earlier studies. First, unlike

studies where a limited number of resumes are used, it lessens (and can test for) the possibility that an employer is reacting to something specific in the particular resume sent out. Additionally, because there is no human interaction with the resume during its creation, the possibility of injecting subjectivity into the process of matching resumes with job openings is completely eliminated. Resumes and resume items (other than the objective) are truly randomly assigned to job openings, eliminating many possibilities for bias.

The computer program used to prepare and match resumes is best explained through example. Say that a job vacancy for a receptionist has been found. The researcher will open the computer program specifying jobs for a receptionist position. The computer program will first randomly choose two of the possible women to apply to the job, for example, Linda Jones (age 45) and Mary E. Smith (age 62). It will then pick an objective statement for Linda ("To obtain a position as a receptionist") and a matching one for Mary ("To secure a position as receptionist"). Similarly it will match work histories and high school. Next it will decide whether or not to test for one or more of the possible reasons for discrimination through adding items to the resume. As an example, to see if lack of energy is a reason employers discriminate against older people, the computer will put under hobbies that Linda Jones is a tennis player, then designate Mary E. Smith as a racquetball player. Regressions found no significant difference between response rates for tennis and racquetball players, or any of the other possible paired choices.

Variations on the resumes ranged as follows. Candidates were named Mary E.

Smith or Linda Jones.[41] The objectives included sales positions, office positions, entry-level nursing positions, wait staff positions and other entry-level or close to entry-level positions that require only a year of combined post-high school education and experience to obtain. All resumes had the applicant currently working at a job. Dates of high school graduation included 1959, 1966, 1971, 1976 and 1986. High schools chosen were Ames High School in Ames City, Iowa and DeKalb High School in DeKalb, IL. Some resumes had experience in computer classes, either from 1986, which makes such experience obsolete, 1996, when the experience is useful but not recent or 2002/2003 when the experience is both useful and recent. Current employment varied as well and ranged from cashier work to secretarial work with a couple of "unusual" jobs possible, such as those giving fork-lift experience. Volunteer work included work at homeless shelters or food banks. Hobbies included some combination of tennis, racquetball, gardening and crafting. An attendance award could also be listed. All resumes had email addresses listed.[42] Appendix Tables 1a and 1b show how resume characteristics were distributed across high school graduation dates.

Typos were introduced to the study in two different ways: First, purposefully coded typographical errors were programmed into the resume machine during the first half of the study when there was more hiring in general. These typos were representative

[41] Mary gets a middle initial because in my experience, and the experience of those I've spoken with, anyone over the age of 30 whose first name is Mary always adds her middle name or middle initial, especially if her last name is also common (unless there's a "Peter, Paul, and..." in front of the Mary). I have not had the same experience with Linda as a first name, although when asked, Linda's middle initial is M.

[42] The census finds that 47% of householders age 45 to 64 have internet access at home (http://www.census.gov/prod/2001pubs/p23-207.pdf). Additionally, places which help people to find work, such as Project Able, strongly encourage applicants to get email addresses and many job finding sites actually take seekers through the steps of signing up for a free hotmail account. Finally, adding an email address to an older resume is likely to work in the older resume's favor, and thus I should find even lower acceptance rates for older workers without adding email addresses.

of those found in actual resumes—they included things like missing punctuation marks, large words that had been misspelled and inconsistent indentation. The second kind of error was inadvertently introduced when applying for a job that did not fit one of the major job categories in the resume program. These errors included things like putting an "a" where an "an" should be or other similar mistakes that native English speakers do not normally make. There are many fewer of these errors and they tend to be most prevalent in Florida and when there was a research assistant regime change.

Call-ins were performed because many entry-level jobs are never advertised via want-ad. I could not use walk-ins because a pilot study showed that, not only were walk-ins time consuming, but many of them generated actual paper job applications with questions whose answers were difficult to control, but hurt an application if left blank, for example, "Describe your ideal job situation." Additionally, there was a worry that a manager would connect the person picking up or turning in an application with the job applicant, rather than looking at the resume characteristics alone. To generate a call-in, a young woman randomly generated an entry in the telephone book. Since large firms tend to have more entries in the telephone book than small firms, and certain industries, such as law offices, tend to have multiple entries, call-ins tend to have a slight bias towards generating these firms. However, they do a better job of generating small firms than want-ads. The company was then called and asked, "Hello, my name is Elizabeth Williams, I was wondering, do you have any entry-level jobs available?" If the person on the phone did not understand, the caller followed with, "Are you hiring for any entry-level positions?" If the person on the phone said no, the caller moved on to another phone book entry. If the person on the phone said yes, the caller tried to elicit a fax

number or email address and later generated a resume and sent it. If there was no fax or email available, the caller first checked to see if there was an online application, and if there was, she sent a resume via that method. Otherwise, the caller coded the company as "no fax/email available" and generated another telephone book entry.

Response rates differ somewhat by method of application as shown in Appendix Table 2. Want-ads are more likely to get both positive and interview responses than Call-ins, faxes slightly more likely than emails. There are some occupational differences in response rates between Massachusetts and Florida. For example, professional/technical non healthcare positions, which are mostly preschool teaching positions, were 1.5 times as likely to hire younger workers in Massachusetts, but there was a much smaller number of positions advertised in Florida, so the sample size could not be compared. There was no difference in age for hiring healthcare workers, mostly Licensed Nurse Practitioners and Certified Nurse Assistants, in Massachusetts, but Florida healthcare agencies were twice as likely to hire younger workers (results not shown). The composition of jobs available differs as well, as can be seen under "firm characteristics" in Appendix Tables 1a and 1b. A quarter of the jobs available in both metropolitan areas were clerical work, but the Boston area was much more likely to hire sales workers, at 24.5% of openings compared to 19.5% in the St. Petersburg-Tampa area. Entry-level professional, education and managerial jobs were also more likely to be advertised in Massachusetts whereas craftsman, operative, service and laborer jobs were more likely to be advertised in Florida.

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Figure 1: Response Rates in Massachusetts

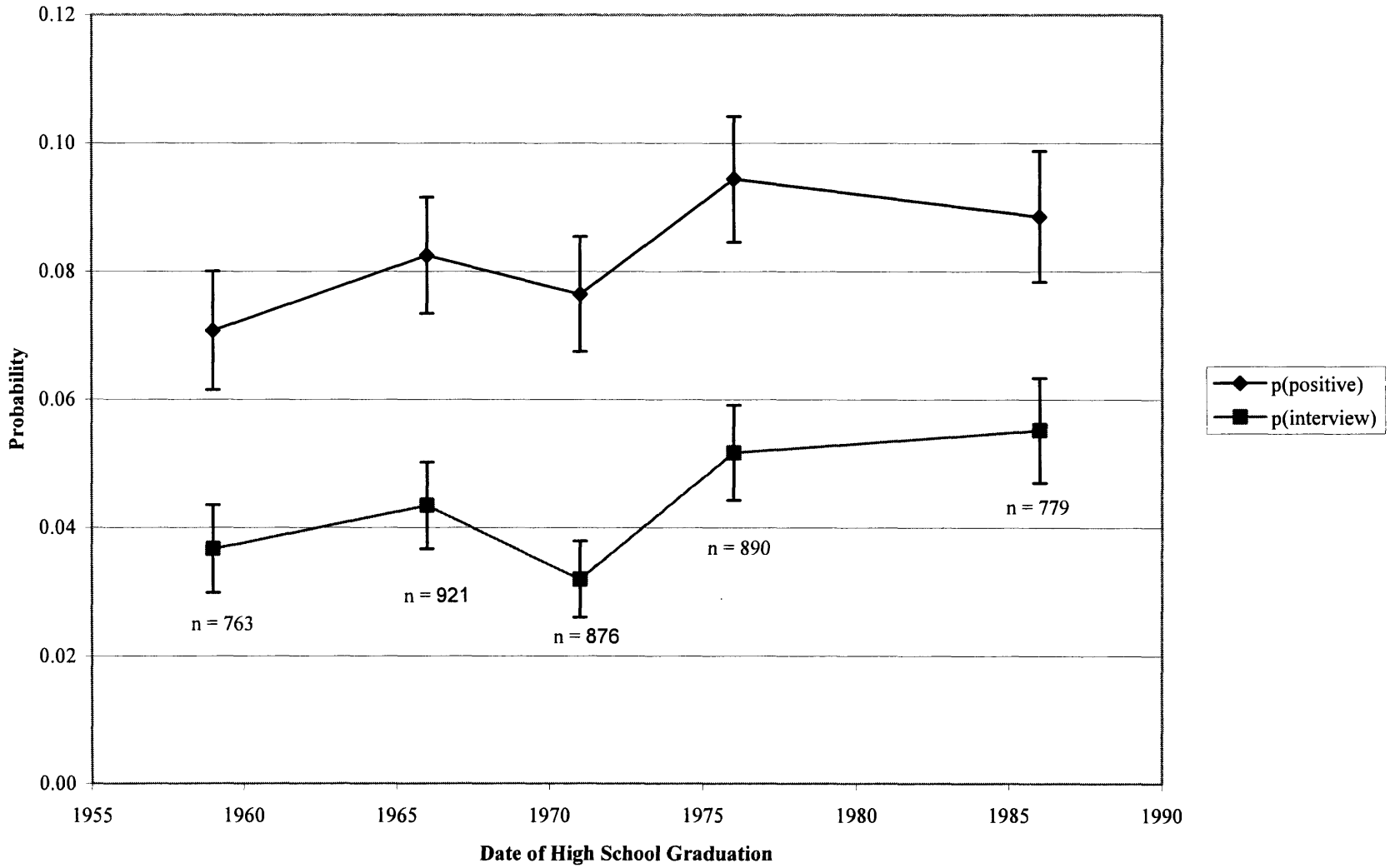

Figure 2: Response Rates in Florida

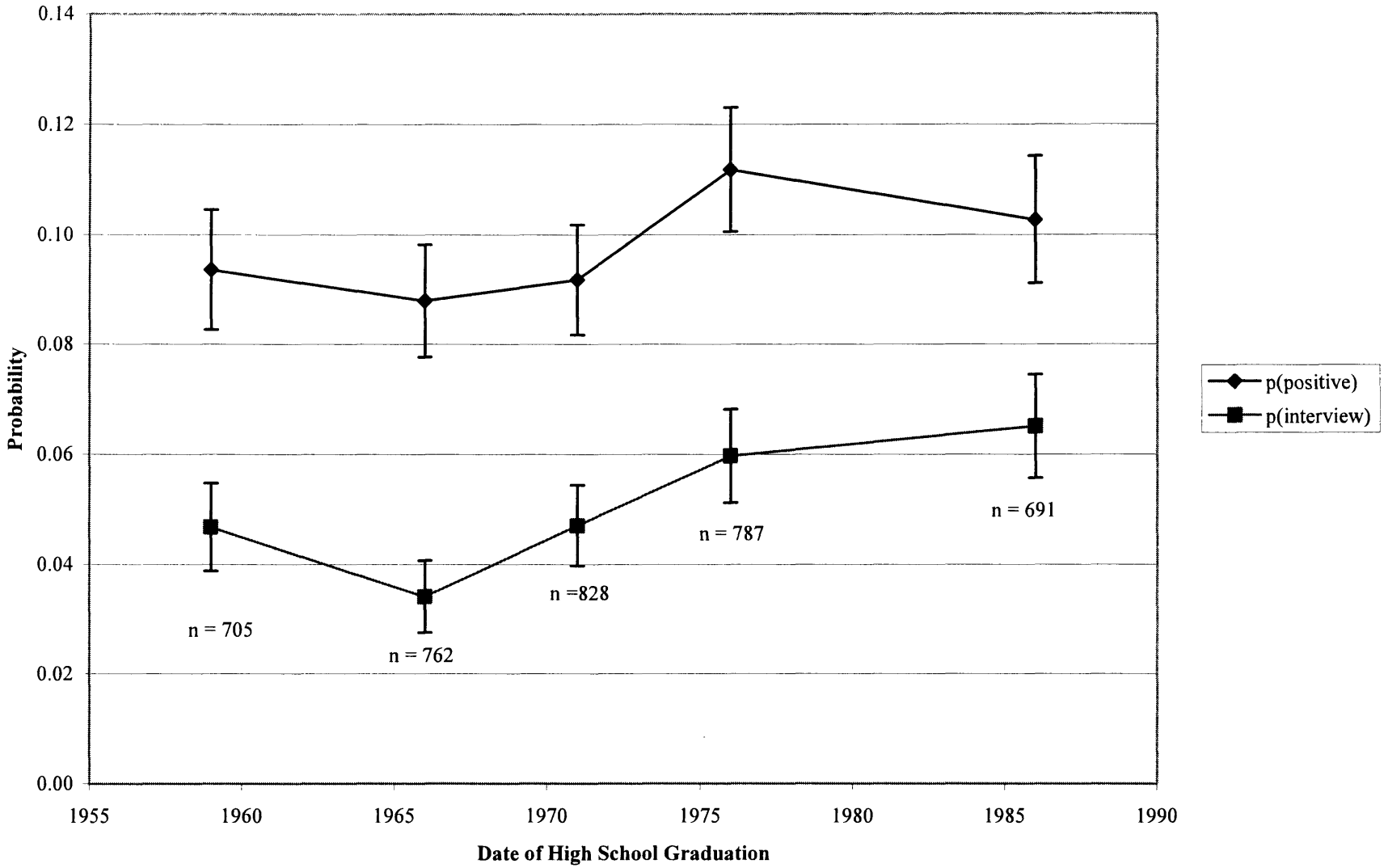

Table 1

Mean Response Rates by Age

	Older		Younger		Difference		# ads needed			
							Older		Younger	
	Positive	Interview	Positive	Interview	Positive	Interview	Positive	Interview	Positive	Interview
MA										
no controls	0.077	0.038	0.092	0.053	0.015	0.016	12.99	26.67	10.91	18.75
	[2560]	[2560]	[1669]	[1669]	(0.09)	(0.01)				
with controls	0.076	0.037	0.092	0.053	0.016	0.016	13.08	27.06	10.87	18.79
	[2564]	[2564]	[1673]	[1673]	(0.07)	(0.01)				
FL										
no controls	0.091	0.043	0.108	0.062	0.017	0.020	10.98	23.42	9.30	16.07
	[2295]	[2295]	[1478]	[1478]	(0.10)	(0.01)				
with controls	0.091	0.042	0.103	0.060	0.013	0.018	11.05	24.07	9.66	16.74
	[2321]	[2321]	[1510]	[1510]	(0.14)	(0.01)				

Notes: Cell number is reported in brackets. P-values are reported in parentheses and refer to two-tailed t-tests.
Controls include years out of 10 in the labor force, years out of 10 in the labor force squared, workgap, college, computer classes since 1996, volunteering, sports, already has insurance, flexible, attendance award, typos, and the following occupational dummies: professional, education, health, manager, sales, craftsman, operative, service, and laborer. For the interview outcome, education and laborer predict failure perfectly and 18 and 133 observations are dropped respectively.

Table 2

Marginal Effect of Age on Likelihood of a Response

	Massachusetts						Florida					
	Positive			Interview			Positive			Interview		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
age		-0.00077			-0.0007			-0.0004			-0.00067	
		(0.0004)			(0.0003)*			(0.0005)			(0.0003)	
older=50+			-0.015			-0.015			-0.014			-0.016
			(0.008)			(0.006)**			(0.009)			(0.007)*
hs59	-0.020			-0.016			-0.002			-0.008		
	(0.011)			(0.007)*			(0.015)			(0.009)		
hs66	-0.007			-0.010			-0.011			-0.023		
	(0.011)			(0.007)			(0.014)			(0.007)**		
hs71	-0.014			-0.020			-0.003			-0.009		
	(0.011)			(0.007)**			(0.014)			(0.008)		
hs76	0.002			-0.004			0.017			0.001		
	(0.012)			(0.008)			(0.015)			(0.009)		
Observations	4229	4229	4229	4229	4229	4229	3773	3773	3773	3622	3622	3755
Ho†	5.08	3.19	3.81	9.80	5.24	7.17	3.09	0.40	2.42	7.99	3.46	5.98
Ho: p-value	(0.2795)	(0.0740)	(0.0509)	(0.0440)	(0.0220)	(0.007)	(0.5436)	(0.5288)	(0.1197)	(0.0918)	(0.0629)	(0.0145)

Notes: Results reported are marginal effects from a probit equation. Standard errors reported in parentheses. The dummy hs86 is omitted. Controls include years out of 10 in the labor force, years out of 10 in the labor force squared, workgap, college, computer classes since 1996, volunteering, sports, already has insurance, flexible, attendance award, typos, and the following occupational dummies: professional, education, health, manager, sales, craftsman, operative, service, and laborer. For the interview outcome, education and laborer predict failure perfectly and 18 and 133 observations are dropped respectively.

* significant at 5%; ** significant at 1%; †Ho: Resume characteristics effects are all zero

Table 3

Marginal Effect of Resume Characteristics on Likelihood of Response

	Massachusetts						Florida					
	Callback			Interview			Callback			Interview		
	All	Older	Younger	All	Older	Younger	All	Older	Younger	All	Older	Younger
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
work gap	-0.013	-0.005	-0.025	-0.005	0.001	-0.014	-0.002	0.009	-0.019	0.002	0.001	0.006
	(0.008)	(0.009)	(0.013)	(0.006)	(0.006)	(0.010)	(0.009)	(0.012)	(0.015)	(0.006)	(0.008)	(0.011)
vocational train.	0.077	0.063	0.100	0.035	0.015	0.081	0.127	0.124	0.138	0.047	0.036	0.070
	(0.019)**	(0.022)**	(0.036)**	(0.014)*	(0.013)	(0.032)*	(0.024)**	(0.031)**	(0.037)**	(0.016)**	(0.020)	(0.028)*
computer	0.007	0.006	0.008	0.010	0.011	0.009	0.013	0.009	0.018	0.018	0.014	0.025
	(0.008)	(0.010)	(0.014)	(0.006)	(0.007)	(0.011)	(0.009)	(0.012)	(0.015)	(0.007)**	(0.008)	(0.012)*
volunteer	0.030	0.027	0.036	0.013	0.015	0.010	-0.001	0.008	-0.015	-0.011	-0.005	-0.020
	(0.008)**	(0.009)**	(0.013)**	(0.006)*	(0.006)*	(0.010)	(0.009)	(0.011)	(0.015)	(0.006)	(0.008)	(0.011)
sports	-0.013	-0.004	-0.026	-0.005	-0.003	-0.007	0.008	0.008	0.008	0.017	0.012	0.026
	(0.008)	(0.009)	(0.013)*	(0.006)	(0.006)	(0.010)	(0.009)	(0.011)	(0.015)	(0.006)**	(0.008)	(0.011)*
insurance	0.016	0.015	0.019	0.009	0.007	0.013	-0.002	-0.005	0.001	-0.007	-0.011	-0.002
	(0.008)*	(0.009)	(0.013)	(0.006)	(0.006)	(0.010)	(0.009)	(0.012)	(0.015)	(0.006)	(0.008)	(0.011)
flexible	-0.010	-0.016	0.000	-0.002	-0.008	0.008	-0.012	-0.009	-0.019	-0.007	-0.009	-0.004
	(0.008)	(0.009)	(0.013)	(0.006)	(0.006)	(0.010)	(0.009)	(0.012)	(0.015)	(0.006)	(0.008)	(0.011)
attendance	0.009	0.004	0.016	0.005	0.003	0.007	0.016	0.016	0.018	0.002	0.001	0.003
	(0.008)	(0.009)	(0.013)	(0.006)	(0.006)	(0.010)	(0.009)	(0.012)	(0.015)	(0.006)	(0.008)	(0.011)
typo	0.011	0.016	0.005	-0.003	0.003	-0.010	0.011	0.003	0.022	0.005	0.004	0.006
	(0.011)	(0.014)	(0.018)	(0.007)	(0.009)	(0.013)	(0.011)	(0.013)	(0.018)	(0.007)	(0.009)	(0.013)
Observations	4229	2560	1669	4229	2560	1669	3773	2279	1478	3755	2267	1427
Ho†	49.87	24.70	29.97	21.55	11.92	17.52	52.16	29.82	28.47	32.27	13.56	22.20
p-value	(0.0000)	(0.0017)	(0.0002)	(0.0058)	(0.1548)	(0.0252)	(0.0000)	(0.0002)	(0.0004)	(0.0001)	(0.0941)	(0.0046)
standard dev.‡	0.056	0.055	0.064	0.031	0.030	0.039	0.067	0.055	0.094	0.042	0.031	0.064

Notes: Results reported are marginal effects from a probit equation. Additional controls not shown are occupational controls for professional, education, healthcare, manager, sales, craftsman, operative, service, laborer and clerical.

* significant at 5%; ** significant at 1%; †Ho: Resume characteristics effects are all zero; ‡ Standard deviation of predicted callback.

Table 4

The Effect of Experience on Interview Requests

	Massachusetts			Florida			Massachusetts			Florida		
	All	Older	Younger	All	Older	Younger	All	Older	Younger	All	Older	Younger
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
same experience	0.016 (0.008)*	0.012 (0.008)	0.025 (0.013)	0.011 (0.008)	0.002 (0.009)	0.029 (0.015)						
experience required							-0.021 (0.005)**	-0.017 (0.006)**	-0.025 (0.009)**	-0.027 (0.006)**	-0.026 (0.007)**	-0.030 (0.011)**
Observation	3651	2207	1444	3266	1980	1168	4228	2560	1668	3755	2267	1427

Notes: Results reported are marginal effects from a probit equation. Standard errors are reported in parentheses. Controls include years out of 10 in labor force, (years out of 10 in labor force squared), workgap, college, computer classes since 1996, volunteering, sports, already has insurance, flexible, attendance award, typos, and the following occupational dummies: professional, education, health, manager, sales, craftsman, operative, service and laborer.

* significant at 5%; ** significant at 1%.

Appendix Table 1a
Summary Statistics: Massachusetts

Variables	All	older	younger	1959	1966	1971	1976	1986
resume characteristics:								
hsgrad	1971.561	1965.625	1980.667	1959.000	1966.000	1971.000	1976.000	1986.000
yrs of 10 in LF	5.590	5.567	5.624	5.468	5.692	5.522	5.697	5.542
typo	0.163	0.171	0.151	0.118	0.198	0.188	0.181	0.117
college	0.196	0.195	0.197	0.204	0.203	0.177	0.212	0.180
computer	0.520	0.523	0.515	0.512	0.533	0.522	0.509	0.521
volunteer	0.504	0.498	0.513	0.509	0.481	0.508	0.520	0.506
sport	0.487	0.483	0.494	0.481	0.493	0.474	0.494	0.493
other hobby	0.196	0.192	0.202	0.210	0.186	0.183	0.199	0.205
insurance	0.503	0.508	0.496	0.528	0.506	0.493	0.487	0.506
flexible	0.517	0.523	0.508	0.533	0.511	0.525	0.515	0.501
attendance	0.493	0.500	0.482	0.499	0.489	0.513	0.480	0.485
recent computer	0.152	0.151	0.154	0.168	0.145	0.142	0.139	0.171
relevant computer	0.378	0.377	0.379	0.377	0.381	0.373	0.376	0.383
age	49.439	55.375	40.333	62	55	50	45	35
method of sending:								
fax	0.786	0.780	0.795	0.790	0.771	0.780	0.816	0.772
email	0.179	0.186	0.170	0.176	0.191	0.188	0.157	0.185
online	0.034	0.034	0.034	0.033	0.037	0.032	0.027	0.041
firm characteristics:								
EOE/AA	0.124	0.127	0.119	0.127	0.135	0.120	0.110	0.130
professional	0.040	0.039	0.040	0.045	0.043	0.031	0.042	0.039
education	0.025	0.028	0.020	0.022	0.033	0.027	0.020	0.021
health	0.140	0.146	0.132	0.144	0.147	0.146	0.145	0.118
manager	0.066	0.062	0.070	0.072	0.054	0.062	0.064	0.076
clerical	0.250	0.252	0.247	0.249	0.256	0.250	0.230	0.266
sales	0.245	0.245	0.244	0.241	0.254	0.240	0.240	0.248
craftsman	0.022	0.021	0.024	0.024	0.017	0.023	0.022	0.026
operative	0.044	0.043	0.046	0.045	0.048	0.037	0.049	0.041
service	0.145	0.140	0.154	0.135	0.126	0.159	0.162	0.145
laborer	0.018	0.019	0.017	0.017	0.015	0.025	0.018	0.017
# obeservations	4229	2560	1669	763	921	876	890	779

Notes:

	hs grad	age
Older includes:	hs1959	62
	hs1966	55
	hs1971	50

	hs grad	age
Younger includes:	hs1976	45
	hs1986	35

Appendix Table 1b
Summary Statistics: Florida

Variables	All	older	younger	1959	1966	1971	1976	1986
resume characteristics:								
hsgrad	1971.538	1965.654	1980.675	1959	1966	1971	1976	1986
yrs of 10 in LF	5.694	5.674	5.726	5.684	5.672	5.667	5.752	5.696
typo	0.259	0.264	0.252	0.206	0.282	0.296	0.288	0.211
college	0.186	0.190	0.179	0.183	0.207	0.180	0.159	0.201
computer	0.511	0.506	0.518	0.506	0.501	0.510	0.522	0.514
volunteer	0.494	0.494	0.493	0.523	0.478	0.484	0.484	0.502
sport	0.499	0.493	0.508	0.495	0.508	0.478	0.513	0.502
other hobby	0.183	0.179	0.188	0.189	0.177	0.173	0.188	0.188
insurance	0.500	0.505	0.493	0.536	0.484	0.496	0.484	0.502
flexible	0.510	0.515	0.502	0.513	0.495	0.536	0.511	0.492
attendance	0.500	0.496	0.505	0.508	0.499	0.483	0.526	0.482
recent computer	0.156	0.152	0.163	0.150	0.151	0.153	0.159	0.168
relevant computer	0.380	0.371	0.393	0.360	0.378	0.373	0.402	0.384
age	49.462	55.346	40.325	62	55	50	45	35
method of sending:								
fax	0.837	0.839	0.834	0.831	0.839	0.847	0.830	0.838
email	0.134	0.136	0.129	0.145	0.134	0.132	0.137	0.120
online	0.029	0.024	0.037	0.024	0.028	0.022	0.033	0.042
firm characteristics:								
EOE/AA	0.149	0.144	0.158	0.152	0.146	0.135	0.146	0.172
professional	0.008	0.007	0.009	0.007	0.005	0.008	0.008	0.010
education	0.005	0.005	0.004	0.004	0.005	0.006	0.005	0.003
health	0.148	0.150	0.144	0.142	0.165	0.144	0.140	0.149
manager	0.048	0.044	0.053	0.051	0.035	0.047	0.058	0.048
clerical	0.262	0.264	0.260	0.247	0.280	0.263	0.263	0.258
sales	0.195	0.189	0.205	0.204	0.180	0.185	0.187	0.226
craftsman	0.042	0.042	0.041	0.043	0.038	0.046	0.044	0.038
operative	0.081	0.081	0.079	0.078	0.085	0.081	0.097	0.059
service	0.173	0.176	0.169	0.180	0.161	0.185	0.165	0.174
laborer	0.035	0.038	0.030	0.044	0.041	0.031	0.029	0.032
# observations	3773	2295	1478	705	762	828	787	691

Notes:

Appendix Table 2a

Response Percentage by Method of Delivery

	Massachusetts			Florida		
	Positive	Interview	# observations	Positive	Interview	# observations
Fax						
Want-Ad	0.09	0.05	2687	0.11	0.06	2508
Call-in	0.06	0.02	636	0.05	0.03	650
All	0.09	0.05	3323	0.10	0.05	3158
Email						
Want-Ad	0.08	0.04	614	0.11	0.05	364
Call-in	0.01	0.01	145	0.06	0.04	140
All	0.07	0.03	759	0.10	0.05	504
Online						
Want-Ad	0.18	0.11	28	0.13	0.13	16
Call-in	0.08	0.03	115	0.04	0.02	95
All	0.10	0.05	143	0.05	0.04	111
All						
Want-Ad	0.09	0.05	3333	0.11	0.06	2888
Call-in	0.05	0.02	896	0.05	0.03	885
All	0.08	0.04	4229	0.10	0.05	3773

Notes:

Appendix Table 2b

Marginal Effect of EOE on Response Rate for Massachusetts

	All Occupations		Non-health Occupations	
Variables	Positive	Interview	Positive	Interview
EOE/AA	0.044	0.027	0.018	0.007
	(0.025)	(0.019)	(0.026)	(0.017)
Over 50	-0.015	-0.018	-0.013	-0.016
	(0.010)	(0.007)	(0.009)	(0.007)
EOE/AA*over 50	-0.009	0.004	-0.024	-0.010
	(0.024)	(0.019)	(0.022)	(0.016)

Notes: Standard errors in parentheses

Chapter Two

State Age Protection Laws and the Age Discrimination in Employment Act

2.1 Introduction

Although the hope is that anti-discrimination laws will raise employment and wages for members of protected groups, a number of studies suggest that these laws may be counter-productive. For example, Gruber (1994) finds that mandates which stipulated that childbirth be covered comprehensively in health insurance plans caused a decrease in wages of women of child bearing age. Similarly, DeLiere (2000), Acemoglu and Angrist (2001), and Jolls and Prescott (2004), among others, find a negative effect on employment prospects for disabled workers following the 1990 Americans with Disabilities Act. In this paper I examine the impact of state and federal legislation meant to protect older workers. The Age Discrimination in Employment Act (ADEA) prohibits discrimination against older workers in hiring, laying off, firing, compensation, or other conditions of employment.

This paper examines whether age discrimination laws have unintended consequences for older workers. There are three margins upon which these laws can affect older workers: firing, hiring and retirement. Employment may increase or decrease for older workers depending on which margins are most affected by the laws. First, a firm affected by these laws will be unlikely to outright fire an older worker for fear of a lawsuit. However, it is very difficult to prove or detect discrimination in hiring, and thus employers may be afraid to hire older workers who will be difficult to fire (Donohue and Siegelman 1991). Finally, since the line between unemployment and retirement tends to blur for older workers (Choi 2002), firms who wish to avoid being sued may increase

retirement incentives for these workers, thus decreasing the employment of older workers. At first examination, increasing retirement rates of older workers may seem as if it benefits both the worker and the company. However, the worker may have actually preferred to stay with the company rather than choose retirement. Because it is difficult for older workers to find new employment, the possibility of losing one's job without the retirement package is a worse prospect for the older worker who may feel that he or she has no choice, than for the younger worker who has a higher probability of finding new employment. Indeed, Schuster and Miller (1984) find that 31% of cases brought under the ADEA before 1981 involved involuntary retirement.

This paper uses state age discrimination laws matched by state and year to the March monthly CPS to look at retirement outcomes for protected workers. To investigate the impact of hiring and job separation outcomes for older workers, I constructed measures of separations and accessions by matching CPS rotation groups as in Bleakley et al. (1999). My empirical strategy uses the assumption that, because of the EEOC backlog, workers in states with their own age discrimination laws are more likely to be affected by the federal law. Workers in states with age discrimination laws have almost twice as long to file and their state FEP office can often process claims more quickly than the EEOC. Thus, I compare workers in states with laws who are affected by the law and workers in states with laws who are not affected by the law to those who are in states without laws.

I find that ADEA laws, including state laws, had no negative effects (using CPS March monthly data) on labor market outcomes before the 1968 federal law was enforced and given to the EEOC in 1979. These laws also do not affect older women or

minorities, possibly because these groups are granted stronger protections under the Civil Rights Act of 1964. After the 1978 legislation, white male workers over the age of 50 in states with ADEA laws worked fewer weeks per year and were less likely to be hired or separated from their jobs, but are more likely to be retired (perhaps involuntarily). These findings suggest that firms do not wish to hire older workers most affected by the law, are afraid to fire older workers, but remove older workers through incentives to retire in states where lawsuits are less of a hurdle for the worker.

The remainder of the paper is organized as follows. Section II provides background information on the legal environment surrounding age discrimination laws, including a brief literature review. Section III provides my empirical strategy. Section IV gives information on data and descriptive statistics. Section V provides results, including robustness checks. Section VI concludes.

2.2 Background

The first state age discrimination law came on the books in 1903 in Colorado. By 1960, eight states had age discrimination laws.[43] Although the US Civil Service had banned maximum hiring ages in federal employment in 1956 and legislated against age discrimination in federal contracting in 1964, federal legislation protecting older workers overall did not appear until 1967 with the introduction of the Age Discrimination in Employment Act, or ADEA. The 1967 ADEA prohibited age based discrimination for those aged 40-65 in firms with 20 or more workers. Under this act, employers were barred from using age in hiring, laying off, firing, compensation, or other conditions of

[43] I have not been able to find any pattern to the introduction of these laws. States with and without laws look very similar across measured characteristics. In the robustness checks portion of the results section I run a test as if states with laws had introduced them 5 years earlier and find no evidence of any underlying differences between states that introduce and have not yet introduced laws.

employment. It also prohibited employers from using age specific language in advertising. Although Adams (2004) finds a small effect of the introduction of this law, most researchers agree that the federal law had little effect until the 1978 amendment to the ADEA[44] (Neumark and Stock 1999). In 1978, congress extended the protected age group to 40-70 and eliminated mandatory retirement for most federal employees. A second major change, in terms of enforcement, came in 1979 when the Department of Labor (and, for federal employment, the US Civil Service Commission) gave administrative responsibility to the US Equal Employment Opportunity Commission (EEOC). Most researchers agree that this change strengthened the power of the ADEA since the change came with an increase in resources and an increase in "pattern and practice" lawsuits (Neumark 2001).

In 1986, congress amended the ADEA to eliminate the upper protected age range for age discrimination, effectively eliminating mandatory retirement for all except in cases where a safety issue related to age might be considered a bona fide occupational qualification (BFOQ), such as for pilots, or where the existence of job tenure would impose an undue hardship on the employer, such as for professors.[45] In 1990, the Older Workers Benefits Protection Act (OWBPA) imposed restrictions on the financial tools employers could use to induce worker retirement (Neumark 2001).

The procedure to file a claim under the ADEA is important, since with its large backlog of cases, the EEOC rarely prosecutes claims itself. If a state has statutes, the claimant must file with the state Fair Employment Practices (FEP) office within 300

[44] Neumark and Stock (1999) note that the existence of the law may have given plaintiffs higher standing in court even in the absence of enforcement mechanisms.
[45] Ashenfelter and Card (2000) looked at the end of mandatory retirement for college faculty.

days, otherwise the claimant must first file with the EEOC within 180 days.[46] The EEOC can then dismiss the claim, at which point the claimant may pursue a civil action in court, or the EEOC can seek to settle or mediate. If the settlement or mediation is unsuccessful, the EEOC can then sue, or if it chooses not to sue, the claimant may sue (Neumark 2001). Over 95% of employment discrimination cases are brought by private attorneys, not the EEOC. Additionally, only 8% of employment discrimination cases filed in federal court proceed to the trial state (Gregory 2001). Since claimants have more time to file if their state has a law, and, because the claim may be processed faster by the state FEP than the backlogged EEOC, claimants in states with age discrimination laws have less of a hurdle to suing than claimants in states without those laws.

Awards are limited to "make whole" status and lawyer fees, that is, the award returns the plaintiff to where he or she would have been had he or she not been the subject of discrimination. These awards include hiring, reinstatement or promotion, back pay and restoration of benefits and lawyers fees. Attorney's fees often make up the bulk of the payment by the firm. Unlike race and gender cases covered by the Civil Rights Act (CRA), additional damages are not awarded except in cases involving willful violation of law and these are limited to twice the amount of actual damages (Levine 1988, Gregory 2001). Thus, suing under the CRA may be more attractive to women and minorities, but the ADEA is the best option for older white men.[47]

The motivation behind the ADEA seems to be that employers incorrectly perceive older workers to be less productive or unwilling to make modest adjustments to accommodate them. Additionally, lawmakers may worry that capable individual older

[46] "For ADEA charges, only state laws extend the filing limit to 300 days." http://www.eeoc.gov/charge/overview_charge_filing.html

[47] The Americans with Disabilities Act was not introduced until 1991.

workers are not granted opportunities based on beliefs about average characteristics of the elderly. Although the labor market fortunes of older workers tend to be better than those of younger workers, older workers are less likely to find employment after being separated from a job (Diamond and Hausman 1984). When older workers do find new jobs, they are clustered into a smaller set of industries and occupations than younger workers (Hutchens 1988).

The majority of people who sue under the ADEA are white male middle managers or professionals over the age of 50. Employment termination in the form of wrongful discharge and involuntary retirement, not differential hiring, is the cause of most suits. It is thus possible that the ADEA acts as a form of employment protection. At the beginning of EEOC enforcement, 14% of claimants were women. By 1995 this number had risen to only 30% (Donohue and Siegelman 1991, Gregory 2001, Schuster and Miller 1984). As mentioned before, it may be that women and minorities have greater protection under the Civil Rights Act, which also allows punitive damages. Thus my identification strategy focuses on white men over the age of 50, who are most likely to sue under the law.

This paper is the first to examine the impact of the ADEA from its early years through a significant time period after its enforcement. It also uses yearly CPS data and examines the effects on many segments of the labor force, not just those over or under the age of retirement. Adams (2004) looks at the introduction of the federal law in 1968 and finds an increase in employment for those protected by the federal law and a decrease for those older than the protected ages. His identification strategy relies on the assumption that states with laws prior to the introduction of the ADEA are not affected by its

passage, an assumption which may or may not be valid since the 1968 ADEA had no enforcement mechanism. There is also some question about the validity of the early CPS which Adams uses in his pre-period. Neumark and Stock (1999) look at censuses from 1940 to 1980, and thus have only one data point after the enforcement of the ADEA.[48] The census may not be the best source of data to examine the impact of these laws since it cannot follow year to year changes.

The end of mandatory retirement in 1986 and 1994 has been more extensively studied than other aspects of the ADEA. Till von Wachter (2002) looks at the shift of mandatory retirement to age 70 in 1978 and its end in 1986 and finds that the labor force participation of workers age 65 and older increases by 10 to 20% by 1986. Mitchell and Luzadis (1988) find that in 1960, pension plans rewarded delayed retirement, but by the 1980s, union plans actively encouraged early retirement. However, non-union plans still rewarded delayed retirement. Ashenfelter and Card (2000) show that the abolition of retirement for college professors in 1994 reduced retirement for those age 70 and 71. Although the end of mandatory retirement is important, it does not tell the story of the entire effect of the ADEA, particularly the consequences of this legislation on older workers wishing to be hired or promoted and the effects on workers who are over the age of 50 (and thus "old") but too young for mandatory retirement to have affected them. This paper fills these gaps in the literature.

2.3 Empirical Strategy

To study the effect of state age discrimination laws, I use an OLS Differences in Differences specification:

[48] I took Neumark and Stock's list of state laws and updated them. In some cases I made corrections, but these corrections to their list were for laws after 1980 and thus do not affect their results.

$$y_{it} = X_i\beta_1 + \beta_2(H_{st} * A_i^{over\,50}) + \beta_3(H_{st} * A_i^{under\,50}) + \theta_t + \partial_a + \varphi_s + \zeta_{st} + \varepsilon_{ist} \qquad (1)$$

where i denotes individuals and t denotes time; y_{it} is weeks worked, a dummy indicating employed, a dummy indicating retirement, a dummy indicating hired this month, or a dummy indicating being separated from a job this month; X_i is a set of controls including a dummy for married and a dummy for high school graduate. H is an indicator that is equal to one if the state s in which the individual resides has an age discrimination law in year t. $A_i^{over\,50}$ is an indicator equal to one if the individual is over the age of 50, and $A_i^{under\,50}$ is an indicator equal to one if the individual is age 50 or under. θ_t is a set of time dummies; φ_s is a set of state dummies; ∂_a is a full set of age dummies; and ζ_{st} is a state specific linear time trend. The assumption behind this strategy is that it is easier for workers to sue, and thus to enforce age discrimination laws, in states that have their own age discrimination laws than in states which do not. Thus workers over the age of 50 in states with laws will be more affected than workers in states without laws.

Equation (1) varies somewhat from the standard differences in differences equation which would be:

$$y_{it} = X_i\gamma_1 + \gamma_2(H_{st}) + \gamma_3(H_{st} * A_i^{over\,50}) + \theta_t + \varphi_s + \partial_a + \zeta_{st} + \varepsilon_{ist}$$

where γ_3 is the effect of the law on workers over the age of 50 compared to workers under the age of 50 in states with laws. This equation is equivalent to equation (1), in that $\beta_2 = \gamma_2 + \gamma_3$ and $\beta_3 = \gamma_2$. The reason for using equation (1), which compares workers over and under the age of 50 in states with laws to workers in states without laws, as the specification, is that one can more clearly see the effects of the law on the two different age groups in the sample. β_2 is the effect of having a law on workers over

the age of 50 and β_3 is the effect of having a law on workers age 50 and under, relative to workers in states without laws. Age 50 was chosen as the age cutoff because white men over 50 are most likely to sue under the law.

A second possible way of identifying is through a Differences in Differences in Differences strategy using women as a second control group. Since women have more to gain from suing under the Civil Rights Act, which allows punitive damages, they may be less likely to sue under age discrimination laws. Additionally, since women's attachment to the labor force is weaker than men's, employers may figure that women will leave or retire on their own before they become a liability due to their age. Finally, women in early cohorts, and thus older women, tend to be much less litigious than men and women in later cohorts. Thus employers may not see older women as constituting as much of a threat due to age discrimination laws as they do men. My strategy is:

$$y_{it} = X_i\beta_1 + \beta_2(M_i * H_{st} * A_i^{over\,50}) + \beta_3(M_i * H_{st} * A_i^{under\,50}) + \beta_4(M_i * A_i^{over\,50}) +$$

$$\beta_5(M_i * A_i^{under\,50}) + \beta_6(M_i * A_i^{over\,50}) + \beta_7(H_{st} * A_i^{over\,50}) + \beta_8(H_{st} * A_i^{under\,50}) + \qquad (2)$$

$$+ \theta_t + \varphi_s + \partial_a + \zeta_{st} + \varepsilon_{ist}$$

where i denotes individuals, t denotes time; y_{it} is weeks worked, a dummy indicating employed, a dummy indicating retirement, a dummy indicating hired this month, or a dummy indicating being separated from a job this month; X_i is a set of controls including a dummy for married and a dummy for high school graduate. M_i is an indicator which equals 1 if the individual is male. H is an indicator that is equal to one if the state s in which the individual resides has an age discrimination law in year t. $A_i^{over\,50}$ is an indicator equal to one if the individual is over the age of 50, and $A_i^{under\,50}$ is an indicator

equal to one if the individual is age 50 or under. θ_t is a set of time dummies; φ_s is a set of state dummies; ∂_a is a full set of age dummies; and ζ_{st} is a linear state time trend. The assumptions behind this strategy are that it is easier for workers to sue, and thus to enforce age discrimination laws, in states that have their own age discrimination laws than in states which do not and that women are less likely to be affected by these laws than men. Thus men over the age of 50 in states with will be more affected than either workers in states without laws or than women.

Finally, I try a more stringent identification strategy in terms of possible state and time trends by allowing state times year effects:

$$y_{it} = \beta_1(H_{st} * A_i^{over\,50}) + \theta_t + \varphi_s + \partial_a + \varphi_s * \theta_t + \varepsilon_{ist} \qquad (3)$$

with variables defined as before.

2.4 Data and Descriptive Statistics

The first sample I use to look at the impact of age discrimination laws is drawn from the 1968-1991 March CPS and is limited to white men aged 25 to 85. I break this set up into two smaller sets, one covering 1968-1977 and the other covering 1978-1991, because congress's committee reported on the ADEA in 1977[49] (amendments followed in 1978 and enforcement by the EEOC in 1979), and because of changes in the CPS beginning in 1976. The impact of the ADEA on employment levels is evaluated by looking at data on weeks worked during the calendar year preceding the March income supplement. The impact on wages is measured using the average weekly earnings, computed using annual earnings data. After 1979, the CPS prompted respondents to be sure to include overtime pay, tips, bonuses, commissions, and money from employers

[49] http://caselaw.lp.findlaw.com/scripts/getcase.pl?court=us&vol=472&invol=353

other than the primary employer. The impact on retirement and labor force participation is measured using the self-reported retirement and labor force coding from the CPS employment status variable. The second sample I use is a matched monthly CPS, which is matched as in Bleakley et al. (1999). I use their algorithm to match job flow variables in order to measure the impact of the ADEA on hiring and job separation outcomes.

CPS questions about weeks worked and income refer to the previous year. The year reported in the tables and figures is the year the CPS was taken, not the year referred to. Questions about labor force status and retirement refer to the respondent's main occupation in the previous week. From 1968 to 1976 in the early period, the CPS does not identify all states but groups some of them together. For state groups in which all states in the group have the same law status for the year, I code these as having or not having the law depending on status. If any state in the group does not have the same status as the others for the year, I drop these states for the years in which they disagree. The basis for state laws was taken from Neumark and Stock (1999) and checked against several secondary sources. When Neumark and Stock (1999) disagreed with the secondary sources, these laws were checked against primary sources from Westlaw and from microfiche and hard copies of compiled state laws. Additionally, the list was updated for years not in Neumark and Stock using Monthly Law Review updates and Westlaw.

Descriptive statistics can be found in Table 1. As mentioned before, the universe is restricted to white males. As workers get older, they are less likely to be unemployed and more likely to be out of the labor force. The employment rate, weekly wage, and total income increase by age until age 45 in the early sample and age 50 in the later

sample, after which they begin to drop. Men in the set are more likely to be married as they get older until their mid-60s. Older cohorts are also less likely to be high school graduates.

2.5 Results

2.5.1 Employment, Wage, and Retirement Effects

Figure 1a plots average weeks worked by white men age 25-49 and those age 50-84 in states with and without laws. The number of weeks worked in Figure 1a is taken as the average of the midpoint of intervalled weeks worked per year. In the later period, there was a redesign in the CPS so that the actual number of weeks worked was recorded, and this is shown in Figure 1b. The number of weeks worked by older men has been declining since 1979 or 1980, although there was a slight uptick in weeks worked by men in states without their own laws in 1984. This decrease is sharper for older men in states with laws and begins in 1977 rather than 1979 for these states. This decline suggests that the possibility of a new enforcement mechanism may have had an effect before the enforcement actually came in place in 1979 in states which were more aware of age discrimination legislation. Weeks worked by younger men dropped as well from 1979 to 1983 and then increased through the rest of the 1980s. In general men in states without laws worked more weeks per year than those in states with laws.

Table 2 reports ordinary least squares (OLS) estimates of equation (1). The universe is white men between the ages of 25 and 85, inclusive. The dependent variables are weeks worked, log weekly earnings and retired.[50] The controls in these regressions are dummies for married and high school graduate, and a set of age dummies, state

[50] The coefficient reported for retired is the marginal effect of the probit.

dummies and year dummies.[51] Regressions are clustered on state. The coefficients of interest are over50* havelaw, which is the interaction of the observation being age 50 or over and being in a state with a law, and under50*havelaw, which is the interaction of the observation being under the age of 50 and being in a state with a law. People in states with laws have more time to file a claim and can work with the state FEP agency rather than directly with the overburdened EEOC; thus they have less of a hurdle to file a lawsuit. Some states with laws also protect workers in firms with fewer than 20 workers. Even though the law covers workers over 40, in practice white men over the age of 50 are the most likely to sue. The table also reports estimates from specifications including a linear time trend interacted with state.

The results in Table 2 suggest a substantial and statistically significant decline in weeks worked per year for people over the age of 50 after it was announced that the ADEA would begin to be enforced in 1978. For example, in Table 2A, column 1 shows a drop of between -1.1 and -1.5 weeks worked for older white men, those over 50, in states with age discrimination laws and essentially no effect on white men under 50 in those states. In the early period, there is no effect on weeks worked for either older or younger workers, though this lack of finding may be due to measurement error in weeks worked per year, since prior to 1976, they were only reported in intervals.

Panel 2B reports estimates on log weekly wages of white men 25-50 and 51-85 in states with and without laws. Once state trends are added, there is no evidence of any effect on either older or younger workers in the early period, although again, since the variable, weekly wages, is manufactured from weeks worked and not all states are

[51] Adding college graduate instead of high school graduate as a control changes the coefficient on over50*havelaw to range between -1.045 without a state year trend to -1.37 with a state trend, compared to -1.5 and -1.16 respectively.

included in the early period, this may be an artifact of the data.[52] In the later period, there is a positive effect on wages of older workers in states with laws, but this effect is not significant. Thus there is little evidence for a wage effect of age discrimination laws.

Panel 2C reports estimates on self-reported retirement of white men over and under 50 in states with and without laws. Note that unlike the weeks worked and income questions, the retirement question is asked about the previous week, rather than the previous year. In the early period, the effect on retirement is positive and just barely significant at the 5% level for older workers, although this effect goes away when state trends are added to the framework. In the later period, older workers are about 4% more likely to retire in states with laws and younger workers are 1.6% less likely to retire and these results are significant at the 1% level. Thus age discrimination laws encourage retirement. This effect could be through two different channels. It could be that companies prefer to offer retirement packages to older workers rather than laying them off or firing them, thus decreasing the chance of a lawsuit. Alternatively, it could be that unemployed older workers who face decreased chances of re-employment prefer to refer to themselves as retired rather than unemployed.

Robustness Checks

Although Figure 1 suggests that the possibility of the enforcement of the federal age discrimination law may have affected employment of older workers in states with laws as early as 1977, when committees reported on the ADEA, an argument can be made for using the year 1978, when the enforcement was announced, or 1979, when the enforcement actually took place as the start year for the later period. Results using these

[52] No evidence is found for an effect in the early period using annual wages either, suggesting that the lack of an effect on weeks worked may be real and not just an artifact of the interval data and missing states.

later cut-off dates can be found in Table 3. Again, the age range refers to the dates the CPS was taken, and thus refers to the earlier year for questions on weeks worked and income. These results are substantively the same as those from 1978-1991, although in general the magnitude of the coefficients is somewhat smaller. Additionally, the results for weeks worked per year lose significance at the 5% level once state trends are included in the regression.

The age 85 was chosen as the top age in order to allow a generous top age specification while still eliminating possible outliers. The typical person who sues under the ADEA, however, is a white male between the ages of 50 and 59. To test for sensitivity to the top age used, I run separate regressions using topcodes of 75, 65, and 59. These results can be found in Table 4. Again, there is no evidence of an age discrimination law effect on relative wages for these smaller age universes. The magnitude of the coefficient of havelaw * 50 drops for both weeks worked per year and retirement as the age universe is trimmed, suggesting there might be a stronger effect on older workers. Weeks worked per year is no longer significant when state time trends are added once 75-84 year olds are removed and loses significance entirely once the universe is restricted below 65, although this result is not unexpected since the universe is smaller. Retirement remains significant for the 25-65 year olds but drops when the range is restricted to those under 60.

Similar regressions shown in Table 5, looking at women and minority groups, found no effect of age discrimination laws on weeks worked. Women and protected minority groups are afforded greater protection under the Civil Rights Act (CRA) and can be awarded punitive damages in addition to "make whole" damages from the CRA, but

not the ADEA. Thus employers may not worry about age for these groups as they are more likely to be sued under the CRA and would have to pay out a larger settlement under the CRA. Additionally, I may be finding no effect because employers may believe that, since women have weaker labor force participation, they may leave before a lawsuit becomes an issue. Finally, women in early cohorts sue less than other groups. Sample sizes for blacks are small and are even smaller for other minority groups and thus may not be big enough to pick up an effect of age laws. I do find a positive effect on weekly wages for black men of all ages in states with laws once state time trends are added in, but that may be a spurious result. I also find an effect on retirement for these groups similar to that of white men and of a slightly larger magnitude. Since retirement is self-defined, it may be that women whose husbands have retired now call themselves retired as well. There may also be spillover effects of encouraging older white men to retire; it is not legal to offer different retirement incentives based on race or gender.

Older white men in middle-management positions are most likely to sue. Therefore it may be of interest to break up the set by college education, since managers are more likely to be college educated. Columns 5-8 of Table 5 report results for white men by college graduation. In table 5A, results on weeks worked for both of these groups are very similar to those of the whole sample, with the coefficient of havelaw*over50 decreasing in magnitude and significance with state trends for the group of non-college graduates but increasing in magnitude and significance for those with a college education. If there is a state-time trend to weeks worked that varies by education, then this would suggest that age discrimination laws do hurt those in demographic groups that are more likely to sue. However, as can be seen in table 5C, retirement effects of

state laws are strongest for those who are not college graduates. In states where it is easier to sue, older non-college graduates are 4.1 to 4.5% more likely to be retired, significant at the 1% level, but college graduates are only 2% more likely to be retired and this finding is not significant. This difference may be due to the smaller size of college graduates in the sample, or it may be that college graduate men are less likely to take retirement packages when offered or to claim retirement when unemployed.

Table 6A reports OLS estimates of equation (2). The universe is all white men and women between the ages of 25 and 85. The dependent variable is weeks worked. The controls in these regressions are dummies for married and high school graduate, and a set of age dummies, state dummies and year dummies. Regressions are clustered on state. The coefficients of interest are male*over50*havelaw, which is the interaction of the observation being male, age 50 or over and being in a state with a law, and male* under50*havelaw, which is the interaction of the observation being male, under the age of 50 and being in a state with a law. Women are less likely to sue under age discrimination laws than men, and as explained above, men in states with laws have less of a hurdle to suing than men in states without laws.

The results in Table 6A agree substantially with the Differences in Differences results for older men using having a law as identification in Table 2. There is still no significant effect of laws for either group prior to the discussion of federal enforcement of the law. In the later period, the magnitude for older men is somewhat larger than the largest estimate in Table 2, with men in states with laws working almost 1.7 fewer weeks using women and not having a law as controls. The triple difference for men under the

age of 50 is negative here whereas in the earlier calculation its sign depended on the inclusion of state trends, though again it is not significant.

Table 6B reports OLS estimates of equation (3). The universe is white men between the ages of 25 and 85. The dependent variable is weeks worked. The controls in these regressions are dummies for married and high school graduate, and a set of age dummies, state dummies and year dummies. Regressions are clustered on state. The coefficients of interest is havelaw*over50, which is the interaction of the observation being age 50 or over and being in a state with a law. These results also find a negative effect on weeks worked for older workers, with older workers working about 1.1 fewer weeks in states with laws. These results are within the bounds of those found by equation (1) presented in Table 2.

On average, there is little clear evidence of an age discrimination law effect on the relative wages of older workers. Therefore the rest of this paper focuses on a further investigation of the employment and labor force participation effects, and the analysis is limited to the demographic groups for which the evidence for employment effects is strongest—white men between the ages of 25 and 85.

Endogeneity of state laws

To test for the possible endogeneity of state laws, in addition to adding state and year effects and trends, I run a specification check looking at the weeks worked outcome at a point 5 years before each state law was passed. The assumption is that employers do not know that a law will be passed prohibiting age discrimination 5 years prior to the law. No evidence is found that having a law in 5 years affects employment or hiring of either older or younger workers. The coefficient for weeks worked per year for older workers

ranges from -0.091 (with no controls) with a standard error (SE) of 0.836 to -0.529 (with controls and a state trend) with an SE of 0.714. Coefficients for younger workers range from -0.330 with an SE of (0.570) to 0.310 with an SE of (0.824). Thus there is no evidence that the introduction of state laws is related to something that directly affects the differential employment of older and younger workers using this test.

2.5.2 The Impact of Age Discrimination Laws on Hiring and Separations

Workers may also be working fewer weeks per year not just because they are more likely to retire but also because they are having difficulty finding work once they have separated from a previous job. Additionally, the law may be helping workers by decreasing fires and layoffs for older workers, since employers do not want to be sued. I used matched CPS rotations groups for the entire year to investigate the effect of age discrimination laws on hiring and separation rates (see Bleakley et al. (1999) for a detailed description of the match). An accession (hire) is recorded when someone who was not employed in month m is employed in month $m+1$. Similarly, an individual is coded as having experienced a separation in month m if he is employed in any month m and not in month $m+1$ (individuals employed in December and not in January are coded as hired or separated in the January year). This definition includes people who move from being employed to no longer being in the labor force as separated, and thus captures those who have voluntarily retired in addition to those subject to layoffs, fires, and other quits. Neither hires nor separations include people who change jobs without leaving employment. These measures of accessions and separations are the same as those used by Bleakley et al.(1999).

As theory would predict, I find that older workers in states with laws are less likely to be hired than workers in states without laws. I also find that workers are less likely to be separated from their jobs, though these results are not significant. Results of a probit using equation (*2*) with Hired and Separated as outcome variables can be found in Table 7. Workers over the age of 50 in states with laws are 5-7% less likely to be hired than workers in states without laws. There is also a small but not significant positive effect on hiring for workers under the age of 50 in these states. Results on job separations are not as clear. There is a trend of reduced job separations for workers over the age of 50 in states with laws and increased job separations for workers under the age of 50, but these results are not significant at the 5% level. Since separations include retirements, which are more likely for older workers in states with laws, I should be picking up two separate effects: increased retirement incentives and decreased firing and layoffs.[53] Still, I find that older workers in states with laws are 1-3% less likely to be separated than workers in states without, and this effect is probably a lower bound.

2.6 Concluding Comments

Employment of workers over the age of 50 has dropped since the ADEA was enforced in 1979. This drop is greater for workers in states where lawsuits are less of a hurdle for older workers, those states with their own age discrimination laws. Workers over the age of 50 in states with laws work between 1 and 1.5 fewer weeks per year than workers in states without laws. This drop in weeks worked may seem high, but it is comparable to the effect that Acemoglu and Angrist (2001) find for the disabled after the

[53] Simply limiting to people who do not say they are retired will not fix this effect since many people who are actually unemployed would call themselves retired for status reasons (Choi 2002).

introduction of the Americans with Disabilities Act (ADA) in 1991, where weeks worked for disabled men fall 1.4 weeks in 1993 and another 1.5 weeks between 1993 and 1995.

Retirement has also increased for these older workers. Older workers in states with laws are 4% more likely to consider themselves retired than workers in states without. Hiring has decreased significantly for older workers in states where it is easier to sue; older workers are 5-7% less likely to be hired in states with laws. Finally, separations have dropped, though not at a significant level.

I find no decline in the employment or retirement for younger workers, non-white workers and female workers. A possible explanation for the difference in findings by race and gender is that before the advent of the ADEA, female and minority workers were already protected by the Civil Rights Act (CRA), which allows for more damages; white men over the age of 50 are the most likely to sue under the ADEA. Additionally, since these groups are not as strongly attached to the labor market, employers may think that they will leave their jobs before possible productivity declines due to age become an issue.

Since the ADEA provides a form of employment protection, it should lead to a lower separation rate for older workers. There does seem to be a protection benefit of this sort, although the results are not conclusive. However, there is also a large effect on increased retirements for these older workers. Employers appear to be reacting to age discrimination legislation and threats of lawsuits by failing to hire older workers, being less likely to fire or lay-off older workers but by trying to remove older workers through retirement incentives. In general, it appears that these age protection laws have had very little effect on workers under the age of 50.

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TABLE 1
Descriptive Statistics for White Men by Age Group

	25-34 (1)	35-44 (2)	45-54 (3)	45-49 (4)	50-54 (5)	55-64 (6)	65-74 (7)	75-84 (8)
				A. 1968-1978				
year	1973.376	1972.990	1973.016	1972.939	1973.096	1973.144	1973.176	1973.072
age	29.222	39.510	49.445	47.001	51.970	59.212	68.958	78.523
employed	0.920	0.940	0.909	0.923	0.894	0.767	0.287	0.115
unemployed	0.042	0.029	0.029	0.029	0.030	0.028	0.013	0.002
married	0.783	0.872	0.878	0.874	0.882	0.860	0.812	0.711
weeks worked	44.698	46.441	45.843	46.386	45.283	40.189	16.444	7.154
high school grad	0.813	0.713	0.626	0.646	0.606	0.504	0.357	0.264
wage income	19048.14	22452.01	21192.14	21909.23	20451.46	16048.99	3521.87	846.12
weekly wage	415.19	472.29	456.79	467.01	446.06	398.32	221.72	122.46
ln(weekly wage)	6.029	6.158	6.124	6.146	6.100	5.987	5.401	4.808
retired	0.000	0.001	0.006	0.003	0.008	0.059	0.486	0.691
Observations	91371	76117	76997	39109	37888	61961	38651	17266
				B. 1978-1991				
year	1985.229	1985.521	1985.124	1985.302	1984.934	1984.989	1985.282	1985.357
age	29.416	39.183	49.380	46.944	51.984	59.370	69.013	78.499
employed	0.887	0.909	0.878	0.895	0.861	0.668	0.217	0.087
unemployed	0.063	0.046	0.040	0.041	0.039	0.032	0.007	0.002
married	0.620	0.782	0.830	0.820	0.841	0.846	0.816	0.738
weeks worked	44.769	46.242	45.299	46.011	44.537	35.813	11.853	4.821
high school grad	0.873	0.854	0.769	0.794	0.742	0.680	0.562	0.430
wage income	16586.65	21652.52	21509.51	22144.00	20831.13	15329.23	2928.75	752.65
weekly wage	358.33	455.66	467.73	473.73	461.12	427.48	249.05	153.54
ln(weekly wage)	5.765	6.034	6.080	6.090	6.068	5.963	5.203	4.751
retired	0.000	0.001	0.010	0.005	0.015	0.130	0.601	0.775
Observations	150194	119063	95009	57414	48748	80812	57522	25208

NOTE: Years refer to survey years. Statistics are weighted using CPS person weights. Income is inflated/deflated to 1982-1984 dollars using the CPI. Summary statistics are taken from the IPUMS CPS, except for data on retirement which is from the Unicon version of the CPS.

Figure 1a
Intervalled Weeks Worked (With Zeros)

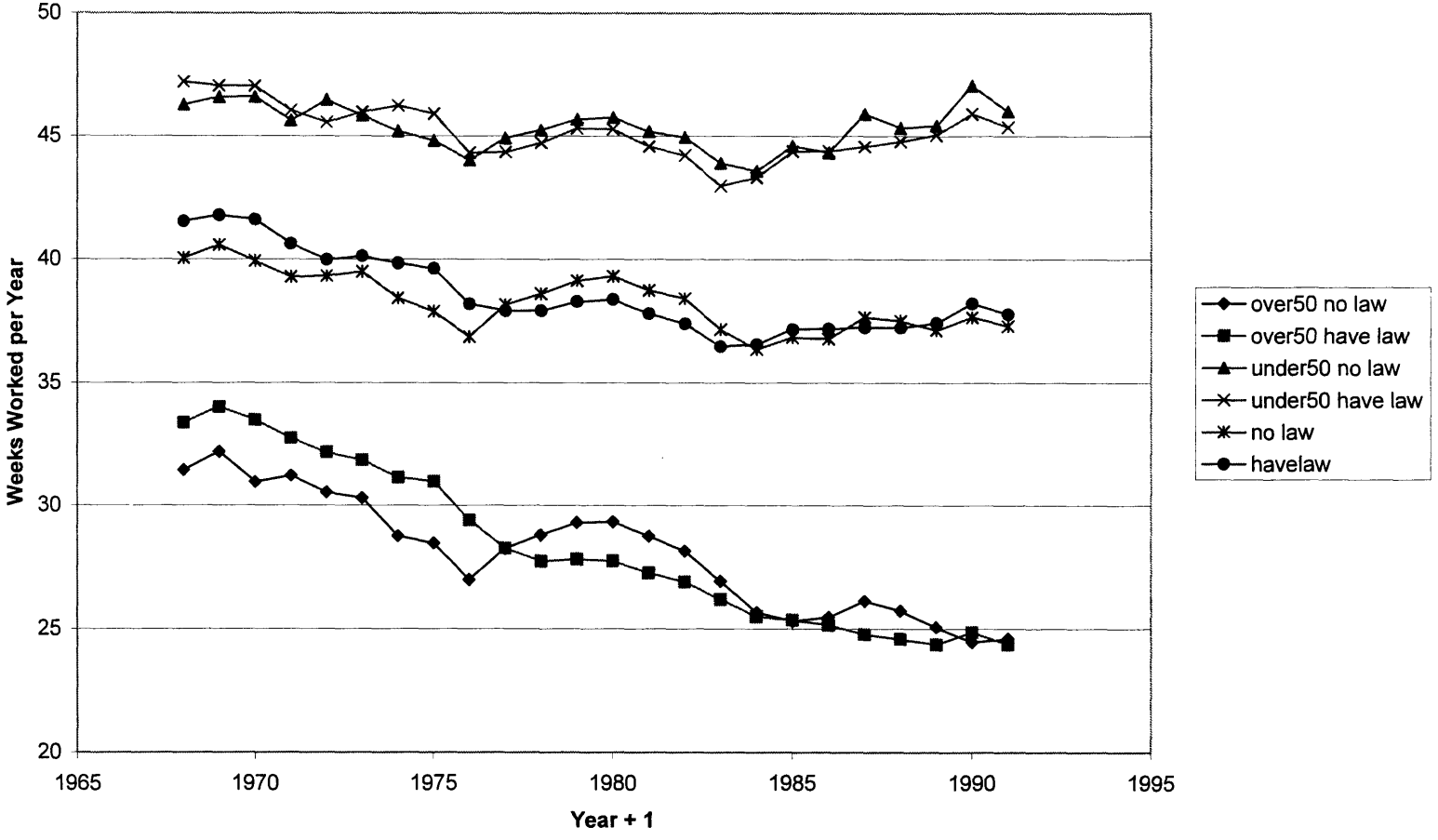

Figure 1b: Weeks Worked by Year

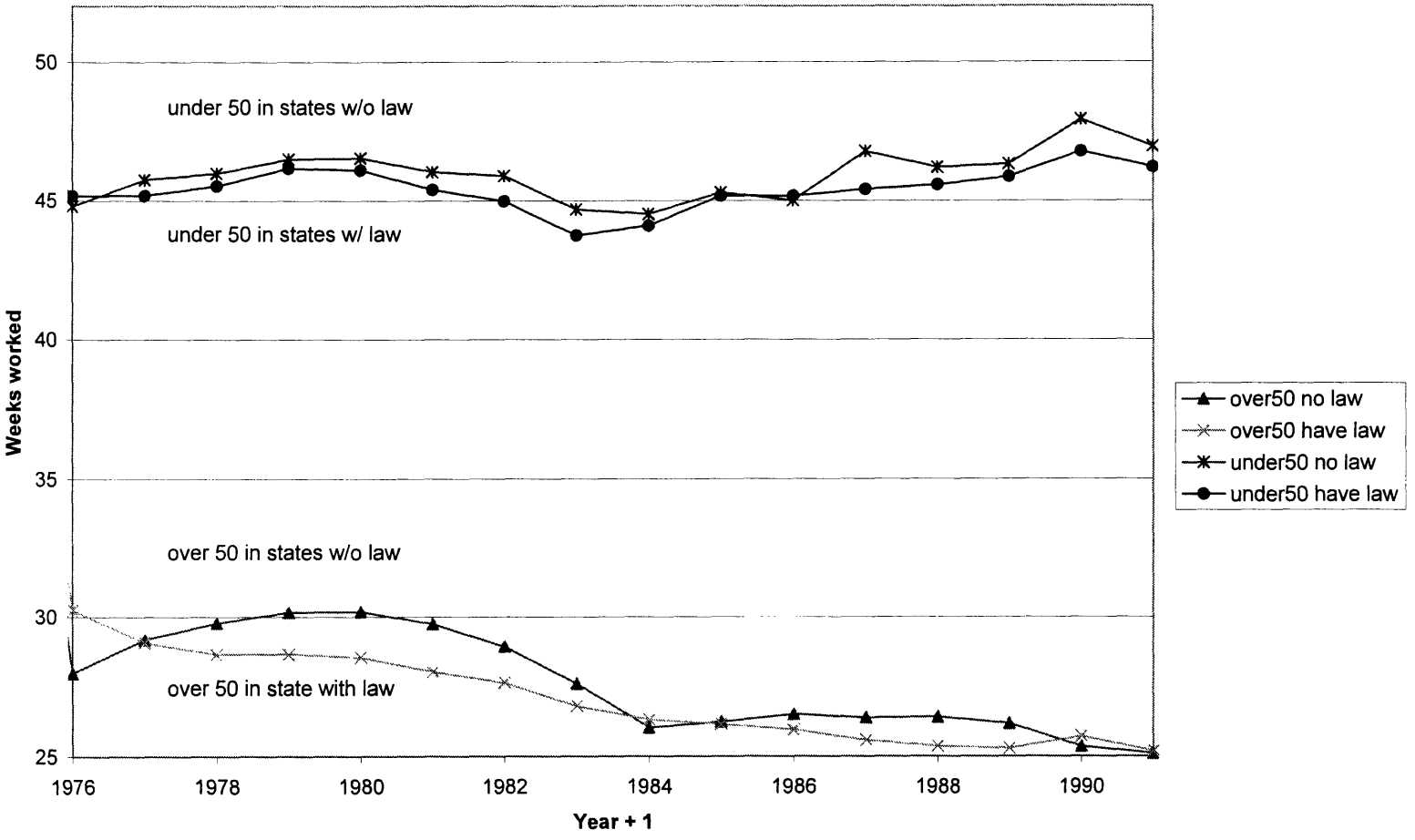

TABLE 2
Initial Results

	A. Weeks Worked per Year			
	1968-1977		1978-1991	
	(1)	(2)	(3)	(4)
havelaw*over50	-0.123	0.180	-1.500	-1.157
	(0.575)	(0.657)	(0.535)**	(0.527)*
havelaw*under50	-0.083	0.219	-0.010	0.326
	(0.744)	(0.474)	(0.441)	(0.510)
Observations	215,912	215,912	558,873	558,873
R-squared	0.40	0.40	0.42	0.42
	B. Log of Weekly Wages			
havelaw*over50	0.081	0.052	0.026	0.074
	(0.039)*	(0.036)	(0.024)	(0.038)
havelaw*under50	0.001	-0.029	-0.007	0.040
	(0.032)	(0.032)	(0.018)	(0.022)
Observations	160,986	160,986	396,442	396,442
R-squared	0.18	0.18	0.16	0.16
	C. Retirement			
havelaw*over50	0.016	0.018	0.042	0.040
	(0.010)	(0.012)	(0.009)**	(0.008)**
havelaw*under50	-0.014	-0.011	-0.015	-0.018
	(0.015)	(0.013)	(0.004)**	(0.006)**
Observations	215989	215989	558,947	558,947
R-squared	0.48	0.48	0.53	0.53
State-year trends?	no	yes	no	yes

NOTES. -- Standard errors are reported in parentheses. The table reports OLS havelaw * over 50 interactions in regressions that include married, high school graduate, age dummies, year dummies, and state dummies. Regressions are clustered on state. The marginal of the Probit coefficient is reported in panel C. Years in charts refer to CPS year. Weeks worked and wage information refer to the previous year, thus 1967-1976 and 1977-1990.
* significant at 5%; ** significant at 1%

TABLE 3
Results by Varying Enforcement Year

	A. Weeks Worked per Year			
	1979-1991		1980-1991	
	(1)	(2)	(3)	(4)
havelaw*over50	-1.269	-0.775	-1.336	-0.753
	(0.564)*	(0.536)	(0.572)*	(0.543)
havelaw*under50	-0.170	0.323	-0.114	0.469
	(0.481)	(0.467)	(0.550)	(0.412)
Observations	522,020	522,020	485,330	485,330
R-squared	0.42	0.42	0.42	0.42
	B. Log of Weekly Wages			
havelaw*over50	0.021	0.078	0.010	0.063
	(0.024)	(0.042)	(0.024)	(0.042)
havelaw*under50	-0.015	0.041	-0.019	0.034
	(0.021)	(0.025)	(0.025)	(0.028)
Observations	370,010	370,010	343,643	343,643
R-squared	0.16	0.16	0.15	0.16
	C. Retirement			
havelaw*over50	0.042	0.038	0.038	0.032
	(0.010)**	(0.009)**	(0.010)**	(0.008)**
havelaw*under50	-0.013	-0.017	-0.013	-0.018
	(0.003)**	(0.005)**	(0.004)**	(0.006)**
Observations	610,730	610,730	484,313	484,313
R-squared	0.55	0.55	0.54	0.54
State-year trends?	no	yes	no	yes

NOTES. -- Standard errors are reported in parentheses. The table reports OLS havelaw * over 50 interactions in regressions that include married, high school graduate, age dummies, year dummies, and state dummies. Regressions are clustered on state. The marginal of the Probit coefficient is reported for panel C. Years in charts refer to CPS year. Weeks worked and wage information refer to the previous year, thus 1978-1990 and 1979-1990.

* significant at 5%; ** significant at 1%

TABLE 4
Results by Varying Top Age Tail: 1978-1991

	A. Weeks Worked per Year					
	25-59		25-64		25-74	
	(1)	(2)	(3)	(4)	(5)	(6)
havelaw*over50	-0.429	-0.374	-0.921	-0.649	-1.471	-1.112
	(0.459)	(0.462)	(0.531)	(0.553)	(0.541)**	(0.559)
havelaw*under50	-0.303	-0.240	-0.111	0.162	-0.034	0.321
	(0.427)	(0.376)	(0.438)	(0.443)	(0.434)	(0.515)
Observations	427,774	427,774	477,124	477,124	535,164	535,164
R-squared	0.07	0.07	0.16	0.16	0.36	0.36
	B. Log of Weekly Wages					
havelaw*over50	0.012	0.066	0.015	0.071	0.021	0.080
	(0.027)	(0.034)	(0.024)	(0.039)	(0.024)	(0.042)
havelaw*under50	-0.017	0.037	-0.013	0.043	-0.013	0.045
	(0.022)	(0.017)*	(0.022)	(0.021)*	(0.021)	(0.024)
Observations	333,097	333,097	357,254	357,254	368,295	368,295
R-squared	0.13	0.13	0.12	0.13	0.15	0.15
	C. Retirement					
havelaw*over50	0.004	0.004	0.017	0.016	0.037	0.034
	(0.004)	(0.004)	(0.005)**	(0.005)**	(0.009)**	(0.008)**
havelaw*under50	-0.002	-0.002	-0.008	-0.009	-0.013	-0.017
	(0.001)	(0.002)	(0.002)**	(0.003)**	(0.004)**	(0.006)**
Observations	427,830	427,830	477,188	477,188	535,232	535,232
R-squared	0.04	0.04	0.22	0.22	0.47	0.47
State-year trends?	no	yes	no	yes	no	yes

NOTES. -- Standard errors are reported in parentheses. The table reports OLS havelaw * over 50 interactions in regressions that include married, high school graduate, age dummies, year dummies, and state dummies. Regressions are clustered on state. The marginal of the Probit coefficient is reported in panel C Years in charts refer to CPS year. Weeks worked and wage information refer to the previous year, thus 1977-1990.

* significant at 5%; ** significant at 1%

TABLE 5

Results by Varying Gender, Race, and Education of Universe, 1978-1991

	A. Weeks Worked per Year							
	White Women		Black Men		Not College Grad		College Grad	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
havelaw*over50	-0.881	-0.130	-0.547	-0.040	-1.495	-0.949	-1.068	-1.583
	(0.556)	(0.389)	(0.927)	(0.738)	(0.490)**	(0.548)	(0.806)	(0.723)*
havelaw*under50	-0.068	0.691	-0.359	0.061	-0.167	0.367	0.459	-0.067
	(0.216)	(0.493)	(0.788)	(0.608)	(0.386)	(0.564)	(0.626)	(0.617)
Observations	620,885	620,885	67,596	67,596	427,817	427,817	131,056	131,056
R-squared	0.27	0.27	0.28	0.28	0.42	0.43	0.34	0.34
	B. Log of Weekly Wages							
havelaw*over50	0.003	0.043	0.107	0.228	0.02	0.058	0.02	0.096
	(0.024)	(0.028)	(0.069)	(0.055)**	(0.021)	(0.031)	(0.036)	(0.070)
havelaw*under50	-0.016	0.022	-0.041	0.077	-0.018	0.020	0.004	0.081
	(0.016)	(0.018)	(0.056)	(0.030)*	(0.019)	(0.016)	(0.021)	(0.052)
Observations	307,795	307,795	44,595	44,595	290,414	290,414	106,028	106,028
R-squared	0.07	0.07	0.19	0.19	0.15	0.15	0.13	0.13
	C. Retirement							
havelaw*over50	0.047	0.054	0.043	0.039	0.045	0.041	0.018	0.023
	(0.007)**	(0.007)**	(0.012)**	(0.015)*	(0.009)**	(0.008)**	(0.012)	(0.012)
havelaw*under50	-0.043	-0.036	-0.017	-0.021	-0.021	-0.024	-0.001	0.004
	(0.006)**	(0.005)**	(0.006)*	(0.011)	(0.005)**	(0.007)**	(0.004)	(0.006)
Observations	620,892	620,892	67,572	67,572	429,678	429,678	129,269	129,269
R-squared	0.33	0.33	0.47	0.47	0.54	0.54	0.47	0.47
State-year trends?	no	yes	no	yes	no	yes	no	yes

NOTES. -- Standard errors are reported in parentheses. The table reports OLS havelaw * over 50 interactions in regressions that include married, high school graduate, age dummies, year dummies, and state dummies. Regressions are clustered on state. Panel C reports the marginal Probit coefficient. Years in charts refer to CPS year. Weeks worked and wage information refer to the previous year, thus 1977-1990. Not College Grad and College Grad columns are white men only.

* significant at 5%; ** significant at 1%

TABLE 6
Alternative Identification Strategies
Weeks Worked

	A. D-D-D Women and Havelaw			
	1968-1977		1978-1991	
	(1)	(2)	(3)	(4)
male*over50*havelaw	**0.366**	**0.366**	**-1.677**	**-1.675**
	(0.673)	**(0.674)**	**(0.552)****	**(0.550)****
male*under50*havelaw	**1.347**	**1.348**	**-1.558**	**-1.555**
	(0.911)	**(0.912)**	**(0.890)**	**(0.889)**
male*over50	15.820	15.818	12.221	12.218
	(0.671)**	(0.671)**	(0.548)**	(0.546)**
male*under50	21.468	21.467	14.099	14.096
	(0.655)**	(0.655)**	(0.812)**	(0.810)**
havelaw*over50	0.250	0.248	-0.370	0.161
	(0.425)	(0.388)	(0.375)	(0.397)
havelaw*under50	-1.058	-1.057	0.735	1.266
	(0.831)	(0.754)	(0.412)	(0.774)
Observations	460122	460122	1179758	1179758
R-squared	0.37	0.37	0.36	0.36
	B. Havelaw * Over50 with State*Time			
havelaw*over50	**0.730**		**-1.151**	
	(0.884)		**(0.485)***	
Observations	460122		1179758	
R-squared	0.19		0.30	
State-year trends?	no	yes	no	yes

NOTES. -- Standard errors are reported in parentheses. Panel A reports OLS male * havelaw * over 50 interactions in regressions that include married, high school graduate, age dummies, year dummies, and state dummies. Panel B reports OLS havelaw * over 50 interactions in regressions that include married, high school graduate, age dummies, year dummies, and state dummies. Regressions are clustered on state. Years in charts refer to CPS year. Weeks worked refers to the previous year, thus 1967-1976 and 1977-1990.
* significant at 5%; ** significant at 1%

TABLE 7
Results on Hiring/Separation Margins: 1978-1991

	A. Hired			
	(1)	(2)	(3)	(4)
havelaw*over50	-0.0680	-0.0548	-0.0714	-0.0577
	(0.0213)**	(0.0211)**	(0.0240)**	(0.0247)*
havelaw*under50	0.0496	0.0455	0.0457	0.0420
	(0.0311)	(0.0306)	(0.0221)*	(0.0222)
Observations	4351023	4351023	4351023	4351023
	B. Separated			
havelaw*over50	-0.0325	-0.0209	-.0258	-0.0135
	(.0213)	(0.0209)	(0.0275)	(0.0274)
havelaw*under50	0.0747	0.0707	0.0805	0.0771
	(0.0364)*	(0.0363)	(0.0483)	(0.0489)
Observations	4351023	4351023	4351023	4351023
Controls?	no	yes	no	yes
State-year trends?	no	no	yes	yes

NOTES. -- Standard errors are reported in parentheses. The table reports the marginal coefficient of havelaw * over 50 interactions in probits that include married, high school graduate, age dummies, year dummies, and state dummies. Marginal effects are reported. (OLS regressions look very similar).
* significant at 5%; ** significant at 1%

Chapter Three

Alleviating Job-Lock? Evidence from a Public Health Care Expansion (with Melissa A. Boyle (MIT))

3.I Introduction

In the United States, unlike in other industrialized nations, the provision of health insurance is characterized by a pooling mechanism that ties insurance to employment. Over ninety percent of private health insurance in the U.S. is employer-provided (Gruber and Madrian, 2002). While firms provide a convenient means of grouping employed individuals in a manner not systematically related to health, it has long been a concern that tying health insurance to employment may have unintended negative consequences. If workers alter their labor supply and retirement decisions because of fears over losing health insurance coverage, or if firms are reluctant to hire certain types of workers (for example, older workers) because of expectations about these workers' insurance costs, inefficiencies may result. If health insurance does impede job mobility, a result that has been termed "job-lock," the most productive employer-employee matches may not be achieved.

Since the early 1990s, a large literature has emerged examining the economic consequences of tying health insurance to employment. While this literature has established a clear relationship between health insurance and labor supply choices, it has suffered from a particular limitation. Because such a substantial proportion of the group insurance market in the United States is tied to employment, it is difficult to find individuals with outside sources of health insurance that are not in some way related to the individuals' employment decisions. Thus, while the consensus in the literature is that reductions in labor mobility result from tying health insurance to employment, the magnitude of this problem has yet to be clearly established.

In addition, the literature has not fully considered whether the introduction of government-sponsored health care may alleviate job-lock. The expansion of public health care programs in the United States will potentially affect the labor supply decisions of program beneficiaries. If workers have an alternative source of health care rather than depending solely on employer-provided insurance, their job mobility may increase.

This paper exploits a change in health care coverage for United States veterans to examine the impact of health insurance on labor supply. A major expansion in both the services offered and the population covered by the Department of Veterans Affairs health care system during the mid-1990s presents a unique opportunity to study the introduction of an exogenous source of health care coverage that is unrelated to employment. This setup both provides a means of cleanly identifying the extent of job-lock, and also demonstrates the potential labor supply effects of expanding other public health insurance programs.

3.2 Predicted Effects of Insurance on Labor Supply

If health insurance were a homogeneous good, firms providing such non-monetary compensation could uniformly reduce wages by the cost of the insurance for all workers choosing to accept such a benefit. In this case, workers could receive the same benefit at the exact same cost in any employment situation, and no labor market distortions would arise. In reality, however, insurance packages vary considerably across firms. This variation arises, in part, from differences across employers in the cost of providing health care coverage. These costs typically vary according to firms'

experience ratings or projected future health expenditures, which depend on factors such as firm size and worker characteristics. Additionally, while health care costs vary substantially across individuals, employers only qualify for favorable tax treatment on health insurance if most workers within the firm are offered equivalent benefits packages. Firms are therefore typically constrained to offering the same package (at the same price) to all employees. For these reasons, health insurance benefits are often not comparable across firms, and workers may not switch to more productive employment situations because of preferences across insurance packages.

For prime-aged workers, health insurance therefore has the potential to impact labor supply in a number of ways. Reluctance to change jobs may result from preferences for the current employer's insurance benefits, and may also arise from fears about the potential effects of a temporary loss of insurance coverage. If coverage lapses while individuals are between jobs, this leaves workers vulnerable to pre-existing conditions exclusions. Additionally, some firms that provide employee health insurance have a waiting period from the start of employment until the time that health insurance benefits become available.

In addition to slowing job mobility, these factors will likely reduce the number of individuals moving into self-employment (since insurance coverage purchased outside the group market is much more expensive). Workers may also be less likely to choose part-time work as a result of the tying of health insurance benefits to employment. Because most part-time jobs do not provide the same benefits as full-time jobs, workers may choose to work full-time to retain health care coverage, even if they would prefer to move into part-time work.

For older workers, health insurance has the same implications as described for younger individuals, and also potentially impacts retirement decisions. As workers age, there are two opposing influences which may affect such a decision. Because older individuals tend to encounter more health problems, work may become more difficult, strengthening the incentive to retire. At the same time, with the increased likelihood of declining health, these individuals may be more concerned about maintaining health insurance coverage in case of large medical expenses. In surveys, older individuals frequently state that they will postpone retirement until they become Medicare-eligible at age 65, even though they would actually prefer to retire earlier (Gruber & Madrian 2002). Additionally, older workers who wish to transition into retirement through part-time work or self-employment may be less likely to do so because they do not want to lose their employer-provided health insurance coverage.

Eligibility for public health care programs has the potential to alleviate many of the possible consequences described above. Public health care will provide beneficiaries with coverage if they are between jobs or if they choose to move into part-time work or self-employment. It will allow individuals wishing to retire before age 65 to do so without sacrificing their health care coverage. Finally, depending on the generosity of a particular public plan, such insurance may be used to fill gaps in employer-provided plans, if an individual's optimal productivity match does not provide the preferred insurance package.

An extensive literature has examined the impact of employer-provided health insurance on the various labor supply decisions described above.[54] Papers examining the impact on prime-aged workers often focus on married individuals (most often wives), and

[54] For a complete review of the literature, see Gruber and Madrian (2002).

compare those with health insurance through their spouses' employers to those without. These studies generally find that the availability of spousal health insurance reduces labor force participation, with the estimated reduction typically falling between 6 and 12 percent.[55] One difficulty with these papers, however, is the required assumption that spousal health insurance is exogenous to an individual's labor supply decisions. Gruber and Madrian (1997) overcome this difficulty by exploiting the introduction of continuation of coverage mandates. These state and federal laws require that employers offer employees the opportunity to continue to purchase health insurance through the employer's plan for up to 18 months after leaving the job.[56] Gruber and Madrian find that these mandates reduce labor force participation by around 15 percent.

Studies that have examined the impact of public insurance on labor supply choices have, thus far, focused on the Medicaid program.[57] The results of this literature are mixed, but in general seem to indicate that for low-income single mothers, the availability of public health care does not have much impact on labor supply. There are a number of difficulties with these studies however, and they cannot clearly identify the potential effects of other expansions in government health care for a number of reasons. First, the necessary focus on the Medicaid-eligible population – mainly low-income single mothers – makes the results less generalizable to other populations. Additionally, the historical tying of Medicaid benefits to cash welfare programs resulted in a unique

[55] e.g. Buchmueller and Valetta (1999), Olson (1998), Schone and Vistnes (2000), Wellington and Cobb-Clark (2000)
[56] The federal law, commonly known as COBRA, was passed in 1986, and requires that individuals be allowed to purchase 18 months of coverage at the average group rate.
[57] For example, Winkler (1991), Moffit and Wolfe (1992), Yelowitz (1995), Yazici (1997), Ham and Shore-Sheppard (2000)

incentive system under which it is extremely difficult to disentangle the effects of Medicaid and welfare.[58]

A number of papers have also studied the impact of employer-provided health insurance on the retirement decision.[59] The majority of these studies suggest a significant effect of health insurance on retirement. Like the literature examining labor supply outcomes for prime-aged workers, the retirement literature has struggled with endogeneity issues. By utilizing continuation of coverage mandates as an exogenous form of outside health care coverage, Gruber and Madrian (1995, 1996) find that the retirement hazard increases by 30% when such coverage becomes available.

The COBRA mandates provided the opportunity to study an exogenous change in outside health insurance coverage, and papers which utilized this control group present the best evidence to date on the effects of health insurance on labor mobility. Even so, the continuation of coverage mandates suffer from two particular shortcomings for this purpose: their relatively short duration and high out-of-pocket costs. The opportunity to study a separate case of an exogenous health care benefit is therefore important not only because of the chance to confirm the results from the COBRA studies, but also to obtain a clearer picture of the magnitude of the effects. Such a case is provided by a radical change in the health care system of the U.S. Department of Veterans Affairs. As an added benefit, this case also mimics the expansion of other public health insurance programs, and therefore provides evidence on the impact of expanding public health care programs on U.S. labor markets.

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[58] Beginning in the 1980s, legislation was introduced to weaken the ties between the two programs. In 1996, the replacement of AFDC with TANF fully decoupled welfare and Medicaid.

[59] For example, Madrian (1994a), Karoly and Rogowski (1994), Hurd and McGarry (1996), Gustman and Steinmeier (1994), Rust and Phelan (1997), Blau and Gilleskie (2001).

3.3 Reforms in the VA Health Care System

Historically, the Department of Veterans Affairs (VA) health care system was a network of hospitals, established over 70 years ago for the purpose of providing specialty care to veterans with conditions resulting from their military service. Over time, the system was expanded to also include care for low-income veterans. VA provided mainly inpatient care, with outpatient services for non-service-connected conditions available only as follow-up to an inpatient stay.

In 1996, the U.S. government began a major overhaul of this health care system. In an effort to catch up with progress in private-sector medicine, VA health care began a shift from an emphasis on hospital-based specialty services to a focus on primary care and preventive medicine. The total number of patients treated in VA hospitals dropped 44 percent between 1989 and 1999, while the total number of outpatient visits increased 66 percent over the same time period (Klein & Stockford, 2001). In addition to this change, VA's resource allocation system was redesigned. Following the HMO model, VA began distributing its health care budget using a capitated, patient-based formula.[60]

As a result of these changes, VA anticipated that increased efficiency would result in significant reductions in costs per patient and in necessary staff. With this in mind, VA felt that it would have the resources available to be accountable to the entire veteran population. VA therefore changed its rules on eligibility for care. Prior to the reform, VA guaranteed care only to veterans with service-connected conditions or low incomes; following the restructuring, all veterans became eligible for VA health care (GAO/T-

[60] In a capitated payment system, the health care provider is reimbursed a flat dollar amount for each patient regardless of the services provided.

HEHS-99-109). As a result of the changes in the system, VA's patient load increased from 2.5 million veterans in 1995 to 4.5 million in 2002.

The VA restructuring affects the availability of health care for the entire veteran population. For non-poor, non-disabled veterans, the policy change constitutes the introduction of a form of non-employer-provided health insurance that was previously unavailable. Even for the previously-eligible (i.e., low-income or disabled) segment of the veteran population, this policy change results in a significant, exogenous change in health insurance status. The VA system following the reorganization became a health care provider much more similar to what was available in the private sector. Thus, even for previous users of VA care, the policy change resulted in the introduction of health care benefits that are much more substitutable for private care than anything provided under the old system. We therefore utilize this exogenous introduction of an outside health insurance option for U.S. veterans to estimate the impact of employer-provided health insurance on labor supply. More generally, this policy change allows us to investigate the effects of increasing the scope and availability of public health insurance programs on individuals' labor supply choices.

3.4 Data and Empirical Model

We use data from the Census Bureau's March Current Population Survey (CPS) for the years 1992 through 2002. We utilize a difference-in-differences estimation strategy to compare the labor supply choices of veterans and non-veterans before and after the restructuring of VA health care. Because of the small number of female veterans and very young veterans in the data, we restrict our sample to include all

surveyed males age 25 and over. The treated population is therefore male veterans age 25 and older, and the control group is male non-veterans over the age of 24. Since changes in VA health care were implemented throughout 1996 and 1997, we define 1992-1995 as the pre-policy period and 1998-2002 as the post-policy period.

The CPS allows us to study labor market outcomes such as retirement, labor force exit, and movement into part-time work or self-employment. In addition to information about employment in the current year, the survey questions individuals about their labor market participation in the previous year. In order to isolate the effect of the policy change on individuals' decisions to alter their labor market behavior, we restrict our sample to those who report working at least one week in the previous year.[61] We use a probit model to estimate the following equation:

(1) $$y_{it} = \beta_0 + \beta_1 veteran_i + \beta_2 veteran_i * post_t + \beta_3 \mathbf{X}_{it} + \delta_t + \mu_{it}$$

where:

y_{it} = labor supply outcomes including: retired, not working, self-employed, working part time

$veteran_i$ = 1 if individual has been honorably discharged from active military duty, 0 otherwise

$post_t$ = 1 in the post-policy period, 0 otherwise

$\mathbf{X}_{it}$ = vector of individual characteristics: age, race, marital status, education, and state dummies, age * veteran dummies, industry and occupation dummies, and indicators for employer-provided health insurance and pensions

δ_t = year dummies

and,

μ_{it} = a random error term.

[61] This strategy is consistent with that used by Gruber and Madrian (1995). We find that restricting our sample to individuals who report working at least 10 weeks in the previous year produces very similar results. Regressions on the whole sample (i.e. including individuals that did not work on the previous year) also produce results that are qualitatively similar, although of smaller magnitude.

Summary statistics are shown in Table 1. Comparing these statistics for the veteran and non-veteran populations reveals that the veteran population is older than the non-veteran population and is aging more rapidly. For this reason, we include an age* veteran interaction term in the regressions, allowing age to enter separately for the two populations.[62] The age difference likely accounts for at least some of the differences in average characteristics between the two groups. Veterans are more likely to be retired or not working and more likely to be married.

3.5 Results

3.5.1 Effects on Labor Force Participation

We begin by examining the effects of the VA policy change on individuals' decisions about whether to participate in the labor force. As mentioned above, the previous literature has found that tying employment to health insurance results in lower retirement rates and less labor mobility. We therefore expect the public insurance expansion implemented by VA to result in a lower probability of labor force participation by veterans.

Table 2 reports results for the labor force participation of both age groups. Reported coefficients are probit marginal effects. All regressions are reported with and without controls for characteristics of the employer in the previous year. These characteristics include dummies for the industry and occupation of employment last year, as well as indicators for whether the individual received health insurance or a pension

[62] One concern with this estimation strategy is the possibility of systematic differences between the treatment and control groups. For this reason, we have also run all reported regressions including veteran interaction terms for every control variable. When we allow all controls to enter for veterans and non-veterans separately, the coefficients on the veteran interactions are typically insignificant, and our coefficient of interest is virtually unchanged.

through his employer. Results are similar with and without these controls, although the magnitude of the coefficient of interest (the coefficient on veteran*post) is generally slightly smaller when employer characteristics are included.

As expected, results consistently show that individuals are more likely to move into not working as a result of the VA policy change. As a result of gaining VA coverage, the probability of working drops by .34 percentage points for a prime-aged worker with average characteristics and by 2.43 percentage points for the average older worker. Relative to the pre-period veteran averages for these two groups, this is about a 4% increase in the probability that a prime-aged worker leaves employment, and a 10% increase in the probability that an older worker ceases work. The introduction of the VA health care benefit increases the probability of entering retirement for older workers by .38 percentage points, a 2.3% increase relative to the pre-period veteran average. While the magnitudes of these estimates are not particularly large, this is likely in part because while we measure the effect on the entire veteran population, only about a quarter of U.S. veterans actually enrolled in the VA system during our study period.[63] The effects are therefore likely to be diluted by the large number of veteran non-users, some of whom may have been unaware of their eligibility to use the VA system.

The results in Table 2 demonstrate a clear relationship between employer-provided health insurance and the choice to participate in the labor market. They also indicate that expanding public insurance programs is likely to lead to earlier retirements

[63] Any veteran wishing to use VA care must first sign-up for benefits or "enroll" in the system. During our study period, some veterans enrolled but did not actually subsequently use VA care. The fact that these individuals enrolled indicates awareness of their eligibility and a potential desire to access the system at a later point in time. It is not clear what proportions of unenrolled veterans are unaware of their eligibility, not interested in ever using VA care, or relying on the option of enrolling at a later date should they desire VA care.

and more labor mobility. We next turn to estimating the effect of health insurance on job choice, by examining movements into self-employment and part-time work.

3.5.2 Effects on Job Choice

Because self-employed workers face much higher insurance costs than those whose insurance is employer-provided and purchased in a group market, individuals who may otherwise wish to move into self-employment may be reluctant to leave jobs with employer-provided insurance. A small number of papers have examined whether this is in fact the case, although the results have been mixed. Holtz-Eakin, Penrod and Rosen (1996) find no effect of employer-provided health insurance on the probability of moving from employment into self-employment, but Madrian and Lefgren (1998) find some evidence that the availability of outside coverage increases movement into self-employment. As reported in Table 3, we find that for prime-aged workers, the probability that the average veteran is self-employed increases by .14 percentage points, an increase of 3.2% relative to the pre-period mean for veterans in this age group. Therefore, as expected, it appears that individuals in this age group are more willing to move into self-employment once relinquishing employer-provided health insurance no longer implies paying high insurance costs out of pocket. At the same time, the sign on the coefficient of interest for older workers is the opposite of expected and, when controls are included for employer characteristics last year, highly significant. Thus, it appears that as a result of the policy change, the average veteran between the ages of 55 and 64 is actually less likely to become self-employed. One possibility is that individuals in this age group were previously using self-employment as a bridge to retirement, so that they

could continue to afford medical care expenses until becoming Medicare-eligible at age 65. Since the rate of retirement increases for this group as a result of the policy change, a potential explanation for these unexpected results could be a substitution of early retirement for self-employment.

In addition to studying the impact of public insurance availability on the probability of self-employment, we also examine the effects of insurance on part-time work. Because most part-time jobs do not provide workers with benefits such as health insurance, workers who place a high value on these benefits may avoid moving into part-time work in order to maintain their health insurance coverage. In surveys, older workers often state that they would prefer to transition into retirement by moving first to part-time employment (Abraham & Houseman 2004). If moving to part-time work means losing health insurance, however, older workers may be reluctant to do so. COBRA may aid in such transitions for individuals within 18 months of attaining Medicare coverage, but COBRA will still not alleviate the full extent of job lock, both because of its high out-of-pocket costs and limited duration. The literature examining the labor supply decisions of prime-aged, married women tests the impact of spousal coverage on the decision to work part time. As mentioned earlier, however, these tests suffer from potential endogeneity bias, since the labor supply decision by husbands and wives is likely to be a joint consideration. Buchmueller and Valletta (1999) find that spousal insurance increases the probability of working in a part-time job by 2.8 to 3.3 percentage points.

Table 4 reports our estimates of the impact of employer-provided insurance on the probability of working part-time. In these regressions, the sample is restricted to individuals employed in the current year. Controls for employer characteristics are

therefore current year controls (as opposed to controls for the previous year, as in the regressions discussed previously). Additionally, we control for whether an employer offers pensions and health insurance, as opposed to whether an individual receives such benefits, because of the fact that many individuals may lose these benefits if they move from full-time to part-time work. As predicted, we find that the average veteran is more likely to work part-time as a result of gaining outside health insurance coverage. For older workers, we estimate a 1.2 percentage point increase in the probability of working part-time, which is a 6.6% increase relative to the pre-period veteran average. Our result for prime-aged workers is also positive and highly significant, but considerably smaller. This result is not surprising, however, since the sample is restricted to men. While the previous literature has found fairly large effects for married women, these individuals are also typically found to have a higher elasticity of labor supply than males. Since prime-aged men are less likely to be secondary earners than prime-aged women, it therefore makes sense that they would be less likely to work part time.

3.5.3 Which Veterans Are Affected?

The previous results consider the effects of the VA policy change on the labor supply of all veterans in particular age groups. We now turn to examining the effects on specific segments of the veteran population, to investigate whether certain groups of veterans are impacted differentially. We test for differences in the impact on low-income and high-income veterans, married and single veterans, and veterans with and without employer-provided health insurance.

As discussed above, certain veterans were eligible for VA health care prior to the policy change. Previously-eligibles (those with service-connected disabilities or low incomes) still have the potential to be affected by the change, since the types of health services available became much more comparable to those covered by employer-provided health insurance. Even so, we would expect to see stronger effects of the policy change on newly-eligible veterans, who go from having no outside insurance to full coverage under the public program. In Table 5, we report results for regressions run on individuals whose household income in the previous year was above or below the VA-established means test cutoffs. All regressions include controls for employer characteristics. There appears to be no effect for low-income veterans on the probability of transitioning to not working, while the effect is strong and positive for higher-income individuals. At the same time, however, 55 to 64 year old veterans in both groups are significantly more likely to retire as a result of the policy change, and the magnitude of the effect is not significantly different across the two groups. Effects on the probability of moving into self-employment or working part-time are similar in most cases for low- and high-income veterans. One exception is that prime-aged low-income veterans are much more likely to move into part-time work as a result of the policy change than their wealthier counterparts, a difference which is significant at the 6% level. This could be, in part, because these individuals are more likely to be disabled, and to therefore desire shorter work hours once they do not need to rely on their employer for insurance.

Table 6 reports results for married and single veterans. VA health care covers only the veteran and not the veteran's spouse or dependents.[64] For this reason, married veterans may still be job-locked if their spouse depends on health insurance provided through the veteran's employer.[65] We therefore expect to find stronger effects of the availability of this public insurance on single veterans than married. As reported in the table, however, this is not always the case. Married veterans are significantly more likely to move into not working or retirement than single veterans. These effects are large, positive and highly significant for married veterans, while the coefficients on veteran*post are insignificant for singles when the outcome is not working, and become significant and negative (although very small) for the retired outcome. The effect of the policy change on the probability of being self-employed is not significantly different across the two groups at either age. Married and single prime-aged workers are also equally likely to move into part-time work, but effects on this outcome are very different for married and single workers approaching the normal retirement age. For older, married workers, the probability of working part time increases by 1.7 percentage points as a result of the policy change. Older, single workers on the other hand, are significantly less likely to be working part time. The probability that these workers hold part-time positions drops by 1.83 percentage points.

The single versus married results are puzzling but have several possible explanations. One factor of importance may be the relationship between marital status

[64] In cases where the veteran is catastrophically disabled or dies as a result of military service, the spouse and other dependents do become eligible for VA care under the CHAMPVA program. This is not relevant in our study, however, as catastrophically disabled veterans will not be in the work force.

[65] It is important to note, however, that a substantial number of married veterans in our sample have working spouses who are more likely to have their own health insurance coverage. Approximately 74% of prime-aged veterans and 57% of older veterans in the sample have wives who are employed.

and household income. As these characteristics are likely to be highly correlated, we may be picking up effects that are not directly related to marital status but rather to wealth. Single veterans may also differ systematically from married veterans according to other unobservable characteristics. For example, these individuals may have different tastes for work than married veterans who are more likely to have families (Coile 2003).

Finally, we cut our sample by individuals who had employer-provided health insurance last year and those that did not. For individuals without employer-provided health insurance, there are not the same potential job-lock effects as for those who have this benefit. At the same time, individuals without group coverage through an employer are still indirectly affected by the U.S. convention of tying insurance to employment, as they pay higher costs to insure out-of-pocket in the individual market than their counterparts with group insurance. Workers without employer-provided insurance may alter their labor supply choices when they become eligible for VA care, because they no longer need to work as much in order to pay for health insurance or out-of-pocket medical expenses. Thus, eligibility for public health insurance may still affect employment behavior for individuals without employer-provided coverage, even though this is not the same job-lock effect typically examined in the literature. It is therefore not clear, a priori, whether to expect significantly different effects across the two insurance status groups.

Table 7 reports results by employer-provided health insurance status in the previous year. For older workers, there is not a statistically significant difference by insurance status in the probability of moving into retirement or not working. There is also no significant difference across the two groups of older workers in the probability of

being self-employed. Older workers without employer-provided health insurance are, however, significantly more likely to be working part-time as a result of the policy change than older workers with a health insurance benefit. For the insured group, the coefficient on post*vet is positive but small and insignificant for the self-employed outcome, while this same coefficient is large, positive, and highly significant for the group without employer-provided health insurance.

For prime-aged workers, there do appear to be differential effects by employer-provided insurance status on both not working and self-employment. The probability that workers in this age group are not working increases by 2 percentage points for those without employer provided insurance, but does not change significantly for those with insurance coverage through their jobs. Workers without insurance coverage are more likely to be self-employed as a result of gaining VA coverage – this probability increases by .2 percentage points. For those with insurance, this coefficient is positive but is extremely small and not highly significant. Finally, for prime-aged workers, there is an increase in the probability of working part-time as a result of the policy change, and this effect is not significantly different by insurance status. It therefore appears overall that the effects of gaining public insurance on labor supply are at least as strong if not stronger for those with no previous insurance coverage through an employer as for those who do have coverage from their place of employment. These results must be interpreted with caution, however, because of potential selection issues. Workers who remain in jobs with health insurance may be less sensitive to the incentives from the policy change than those that do not. For this reason, it would be more ideal to cut the sample according to

health insurance status prior to 1996. Since this information is not available, however, we are only able to base our samples on health insurance status in the previous year.

3.6 Concluding Comments

This paper demonstrates a strong relationship between health insurance and labor supply choices. As with the literature that examines the introduction of COBRA and other continuation of coverage mandates, we utilize an exogenous change in outside health insurance status to show that tying health insurance to employment reduces job mobility, resulting in potential inefficiencies in the labor market. By utilizing a major organizational change in VA health care, we are also able to estimate the effects of expanding public health insurance availability on labor supply choices.

Our results demonstrate a significant effect of public health insurance on work decisions. We find particularly strong results for those workers in the 55-64 year old age group, who are approaching the normal retirement age. For this age group, our results suggest a positive and significant increase in early retirement with the availability of outside health care coverage. Our effects appear smaller than those found by Gruber and Madrian (1995), which is likely at least partially explained by the different populations considered by the two studies.[66] In addition, our results can generally be considered to be a lower bound on the effects of other public insurance expansions on labor supply, because while the VA expansion potentially extends benefits to a huge population of individuals, only about 25% of eligibles expressed an interest in using the program during our study period.

[66] Because they are estimating the effects of continuation of coverage mandates, Gruber and Madrian restrict their sample to individuals with employer-provided health insurance in the previous period.

For veterans in both age groups, effects on labor force participation appear slightly stronger for higher-income indivduals who are more likely to be newly-eligible for VA care. Effects also appear somewhat stronger for married than for single veterans. Finally, the availability of public insurance affects labor supply choices of individuals with and without employer provided health care coverage.

Overall, our study confirms the job-lock effects of tying health insurance to employment, and suggests that public health insurance expansions have the potential to alleviate some of the reductions in job mobility caused by this type of health insurance regime. While the magnitudes of our results are relatively small, they are likely diluted because we consider the impact on the labor supply of all veterans, many of whom may never consider using the VA program. This evidence therefore suggests even larger potential for the alleviation of job-lock through publicly provided health care.

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Table 1A. Summary Statistics, CPS 1992-2002
(Sample restricted to men between 55 and 64 employed last year)

	Veterans		Non-Veterans	
	Pre (N=7775)	Post (N=8242)	Pre (N=6258)	Post (N=10820)
age	59.365	58.843	58.480	58.662
married	0.813	0.804	0.803	0.791
white	0.927	0.907	0.845	0.844
no hs	0.142	0.062	0.294	0.209
hs	0.352	0.352	0.310	0.298
some coll	0.240	0.294	0.151	0.178
coll grad	0.160	0.171	0.112	0.152
grad sch	0.106	0.120	0.133	0.163
pension plan	0.480	0.545	0.448	0.490
included in pen plan	0.429	0.487	0.403	0.439
inc. in emp HI plan	0.628	0.653	0.583	0.596
Northeast	0.236	0.219	0.259	0.226
Midwest	0.257	0.247	0.246	0.231
South	0.286	0.278	0.292	0.303
West	0.222	0.257	0.204	0.240
not working	0.251	0.224	0.227	0.199
part time|employed*	0.182	0.175	0.171	0.155
retired**	0.163	0.148	0.119	0.107
self-employed	0.039	0.040	0.033	0.039
Occupations:				
prof/manag	0.254	0.267	0.249	0.288
tech/sales/clerical	0.175	0.174	0.143	0.139
service	0.065	0.069	0.082	0.077
farming	0.041	0.030	0.058	0.048
craftsman	0.135	0.140	0.132	0.127
operator	0.136	0.127	0.163	0.151
Industries:				
agriculture/mining	0.045	0.034	0.060	0.053
construction	0.068	0.071	0.079	0.082
manufacturing	0.159	0.145	0.181	0.149
transport/commun	0.078	0.098	0.064	0.067
trade	0.136	0.125	0.149	0.131
financial/real estate	0.050	0.053	0.046	0.050
business/repair	0.045	0.051	0.045	0.055
personal	0.033	0.028	0.031	0.038
public	0.052	0.066	0.032	0.033
professional	0.139	0.134	0.140	0.173

*Part-time statistics are based on being currently employed. There are 3712 observations for pre-veterans, 8181 post-veterans, 3407 pre-non-veterans, and 11225 post-non-veterans.
** Number of observations for Retired is 3678 for pre-veterans and 3233 for pre-non-veterans, because variable does not exist for 1992-1993.

Table 1B. Summary Statistics, CPS 1992-2002
(Sample restricted to men between 25 and 51 employed last year)

	Veterans		Non-Veterans	
	Pre (N=19091)	**Post (N=16199)**	**Pre (N=74844)**	**Post (N=99620)**
age	41.507	41.395	36.091	37.683
married	0.699	0.669	0.630	0.627
white	0.884	0.845	0.865	0.859
no hs	0.047	0.033	0.131	0.125
hs	0.379	0.371	0.326	0.321
some coll	0.355	0.396	0.246	0.256
coll grad	0.144	0.142	0.198	0.206
grad sch	0.075	0.057	0.099	0.092
pension plan	0.615	0.662	0.537	0.580
included in pen plan	0.536	0.566	0.453	0.495
inc. in emp HI plan	0.647	0.673	0.612	0.626
Northeast	0.197	0.169	0.242	0.212
Midwest	0.249	0.248	0.240	0.241
South	0.308	0.317	0.286	0.284
West	0.246	0.266	0.232	0.264
not working	0.092	0.082	0.080	0.064
part time|employed*	0.116	0.104	0.117	0.106
retired**	0.005	0.005	0.001	0.002
self-employed	0.044	0.043	0.037	0.032
Occupations:				
prof/manag	0.241	0.237	0.271	0.289
tech/sales/clerical	0.202	0.185	0.192	0.177
service	0.095	0.102	0.086	0.087
farming	0.023	0.018	0.040	0.036
craftsman	0.208	0.218	0.190	0.194
operator	0.189	0.189	0.190	0.182
Industries:				
agriculture/mining	0.033	0.028	0.049	0.044
construction	0.098	0.106	0.111	0.123
manufacturing	0.204	0.187	0.207	0.185
transport/commun	0.135	0.140	0.085	0.089
trade	0.144	0.138	0.183	0.180
financial/real estate	0.040	0.037	0.048	0.049
business/repair	0.064	0.073	0.066	0.079
personal	0.026	0.031	0.036	0.038
public	0.095	0.093	0.045	0.044
professional	0.121	0.117	0.139	0.133

*Part-time statistics are based on being currently employed. There are 9393 observations for pre-veterans, 17040 post-veterans, 40197 pre-non-veterans, and 109688 post-non-veterans.
** Retired information is not available for 1992 and 1993. Therefore there are 8948 veterans and 37064 non-veterans in the pre-period.

Table 2. Not Working and Retired Outcomes by Age Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Not Working, Employed Last Yr., 55-64	Not Working, Employed Last Yr., 55-64	Not Working, Employed Last Yr., 25-51	Not Working, Employed Last Yr., 25-51	Retired, Employed Last Yr., 55-64	Retired, Employed Last Yr., 55-64
veteran	0.0281	0.0125	0.0114	0.0116+	0.0199+	0.0123**
	(0.0224)	(0.0164)	(0.0086)	(0.0061)	(0.0106)	(0.0038)
veteran x post	**0.0184****	**0.0243****	**0.0061***	**0.0034***	**0.0141****	**0.0038****
	(0.0061)	**(0.0042)**	**(0.0026)**	**(0.0015)**	**(0.0032)**	**(0.0012)**
married	0.1099**	0.0251**	-0.0160**	-0.0133**	0.0763**	0.0143**
	(0.0036)	(0.0046)	(0.0020)	(0.0011)	(0.0039)	(0.0026)
non-white	-0.0494**	-0.0224**	-0.0414**	-0.0185**	-0.0172*	0.0018
	(0.0067)	(0.0067)	(0.0029)	(0.0022)	(0.0073)	(0.0035)
pension		-0.1327**		-0.0413**		-0.0386**
		(0.0055)		(0.0010)		(0.0025)
healthins		-0.0374**		-0.0487**		0.0165**
		(0.0082)		(0.0022)		(0.0015)
industry/occ?	No	Yes	No	Yes	No	Yes
Obs	32721	32721	207611	207611	25666	25666

Notes: Coefficients estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. Regressions include age, age*veteran, state, year and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. Regression universe is restricted to men who were employed at least one week in the year prior to the survey year.

+ significant at 10%; * significant at 5%; ** significant at 1%

Table 3. Self-Employed Given Employed Last Year

	(1)	(2)	(3)	(4)
	55-64	55-64	25-51	25-51
veteran	-0.0043	-0.0012	-0.0016	-0.0020*
	(0.0093)	(0.0026)	(0.0045)	(0.0010)
veteran x post	**-0.0030+**	**-0.0014****	**0.0046****	**0.0014****
	(0.0016)	**(0.0005)**	**(0.0013)**	**(0.0004)**
married	-0.0040	-0.0008	-0.0001	-0.0011**
	(0.0026)	(0.0007)	(0.0006)	(0.0002)
non-white	-0.0087**	-0.0012	-0.0110**	-0.0028**
	(0.0031)	(0.0008)	(0.0012)	(0.0003)
pension		0.0135**		0.0060**
		(0.0017)		(0.0005)
healthins		0.0040**		0.0038**
		(0.0007)		(0.0004)
industry/occ?	No	Yes	No	Yes
Obs	32721	32721	207611	207611

Notes: Coefficients estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. Regressions include age, age*veteran, state, year and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. Regression universe is restricted to men who were employed at least one week in the year prior to the survey year.

+ significant at 10%; * significant at 5%; ** significant at 1%

Table 4. Part-Time Work if Employed

	(1)	(2)	(3)	(4)
	55-64	55-64	25-51	25-51
veteran	0.0070	0.0060	-0.0082+	-0.0052
	(0.0090)	(0.0080)	(0.0046)	(0.0035)
veteran x post	**0.0104***	**0.0118****	**0.0032**	**0.0043****
	(0.0043)	**(0.0042)**	**(0.0020)**	**(0.0015)**
married	-0.0286**	-0.0151**	-0.0392**	-0.0241**
	(0.0052)	(0.0040)	(0.0012)	(0.0010)
non-white	0.0036	0.0117*	-0.0159**	-0.0065**
	(0.0060)	(0.0050)	(0.0015)	(0.0011)
pension		-0.0487**		-0.0119**
		(0.0034)		(0.0009)
healthins		-0.0450**		-0.0472**
		(0.0041)		(0.0011)
industry/occ?	No	Yes	No	Yes
Obs	32102	32102	218928	218928

Notes: Coefficients estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. Regressions include age, age*veteran, state, year and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. Regression universe is restricted to men who are currently employed in the survey year.

+ significant at 10%; * significant at 5%; ** significant at 1%

Table 5. Results by Estimated Means Test Cutoff

a. Above Means Test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Not Working, Employed Last Yr., 55-64	Not Working, Employed Last Yr., 25-51	Retired, Employed Last Yr., 55-64	Self-Employed, Employed Last Yr., 55-64	Self-Employed, Employed Last Yr., 25-51	Part-time, Employed this Yr., 55-64	Part-time, Employed this Yr., 25-51
veteran	0.0104	0.0092*	0.0110**	-0.0021	-0.0019+	0.0012	0.0008
	(0.0157)	(0.0046)	(0.0042)	(0.0033)	(0.0011)	(0.0083)	(0.0036)
veteran x post	0.0275**	0.0048**	0.0043**	-0.0016**	0.0012**	0.0099**	0.0012
	(0.0042)	(0.0016)	(0.0012)	(0.0006)	(0.0004)	(0.0037)	(0.0009)
Obs	27677	167775	21781	27677	167775	27554	176316

b. Below Means Test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Not Working, Employed Last Yr., 55-64	Not Working, Employed Last Yr., 25-51	Retired, Employed Last Yr., 55-64	Self-Employed, Employed Last Yr., 55-64	Self-Employed, employed Last Yr., 25-51	Part-time, Employed this Yr., 55-64	Part-time, Employed this Yr., 25-51
veteran	0.0242	0.0389	0.0203	0.0017	-0.0020*	0.0636+	-0.0480**
	(0.0568)	(0.0339)	(0.0155)	(0.0041)	(0.0009)	(0.0372)	(0.0133)
veteran x post	-0.0189	-0.0074	0.0069+	-0.0019	0.0011*	0.0054	0.0185*
	(0.0205)	(0.0071)	(0.0038)	(0.0016)	(0.0004)	(0.0166)	(0.0079)
Joint Sig?	Yes	Yes	No	No	No	No	No
Obs	5044	39836	3885	2907	39836	4548	42612

Notes: Coefficients estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. The regression universe in panel a is restricted to those persons who are above the income means test (given number of children under the age of 18) needed to meet the VA requirement prior to the reform. The regression universe in panel b is restricted to those below the same income means test. Columns (1) - (5) are restricted to those who worked at least one week in the year prior to the survey. Columns (6) - (7) are restricted to those currently employed in the survey year. Regressions include age, age*veteran, state, year and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. "Joint Sig?" reports whether the veteran*post coefficients for the two populations are statistically significantly different from one another at the 5% level.

+ significant at 10%; * significant at 5%; ** significant at 1%

Table 6. Results By Marital Status

a. Married

	(1) Not Working, Employed Last Yr., 55-64	(2) Not Working, Employed Last Yr., 25-51	(3) Retired, Employed Last Yr., 55-64	(4) Self-Employed, Employed Last Yr., 55-64	(5) Self-Employed, employed Last Yr., 25-51	(6) Part-time, Employed this Yr., 55-64	(7) Part-time, Employed this Yr., 25-51
veteran	0.0039	0.0068	0.0122*	-0.0015	-0.0013	-0.0013	0.0004
	(0.0216)	(0.0055)	(0.0052)	(0.0022)	(0.0010)	(0.0093)	(0.0042)
veteran x post	**0.0325****	**0.0057****	**0.0079****	**-0.0017****	**0.0011***	**0.0174****	**0.0035***
	(0.0042)	**(0.0015)**	**(0.0015)**	**(0.0007)**	**(0.0005)**	**(0.0045)**	**(0.0015)**
Obs	26221	132607	20528	26221	132607	26760	152739

b. Single

	(1) Not Working, Employed Last Yr., 55-64	(2) Not Working, Employed Last Yr., 25-51	(3) Retired, Employed Last Yr., 55-64	(4) Self-Employed, Employed Last Yr., 55-64	(5) Self-Employed, employed Last Yr., 25-51	(6) Part-time, Employed this Yr., 55-64	(7) Part-time, Employed this Yr., 25-51
veteran	0.0228	0.0209	0.0050	0.0010	-0.0030+	0.0568**	-0.0274**
	(0.0234)	(0.0173)	(0.0074)	(0.0057)	(0.0016)	(0.0209)	(0.0065)
veteran x post	**0.0055**	**-0.0043**	**-0.0061***	**0.0016**	**0.0013***	**-0.0183***	**0.0076***
	(0.0070)	**(0.0033)**	**(0.0026)**	**(0.0022)**	**(0.0006)**	**(0.0093)**	**(0.0038)**
Joint Sig?	Yes	Yes	Yes	No	No	Yes	No
Obs	6500	75004	5138	5909	75004	5342	66189

Notes: Coefficients estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. The regression universe in panel a is restricted to married men. The universe in panel b is restricted to not married men. Columns (1) - (5) are restricted to those who worked at least one week last year. Columns (6) - (7) are restricted to those currently employed. Regressions include age, age*veteran, state, year and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. "Joint Sig?" reports whether the veteran*post coefficients for the two populations are statistically significantly different from one another at the 5% level.

+ significant at 10%; * significant at 5%; ** significant at 1%

Table 7. Results by Health Insurance Status

a. Employer-Provided Health Insurance Last Year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Not Working, Employed Last Yr., 55-64	Not Working, Employed Last Yr., 25-51	Retired, Employed Last Yr., 55-64	Self-Employed, Employed Last Yr., 55-64	Self-Employed, employed Last Yr., 25-51	Part-time, Employed this Yr., 55-64	Part-time, Employed this Yr., 25-51
veteran	0.0129	0.0187**	0.0060	-0.0032	-0.0029+	0.0132	0.0002
	(0.0148)	(0.0042)	(0.0077)	(0.0052)	(0.0018)	(0.0097)	(0.0024)
veteran x post	**0.0195****	**-0.0018**	**0.0037***	**-0.0027****	**0.0009+**	**0.0003**	**0.0023***
	(0.0049)	**(0.0013)**	**(0.0018)**	**(0.0008)**	**(0.0005)**	**(0.0024)**	**(0.0011)**
Obs	20092	129917	15822	20092	129917	21857	144906

b. No Employer-Provided Health Insurance Last Year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Not Working, Employed Last Yr., 55-64	Not Working, Employed Last Yr., 25-51	Retired, Employed Last Yr., 55-64	Self-Employed, Employed Last Yr., 55-64	Self-Employed, employed Last Yr., 25-51	Part-time, Employed this Yr., 55-64	Part-time, Employed this Yr., 25-51
veteran	0.0080	-0.0083	0.0240*	0.0000	-0.0004	-0.0162	-0.0244*
	(0.0276)	(0.0184)	(0.0097)	(0.0025)	(0.0008)	(0.0216)	(0.0101)
veteran x post	**0.0276****	**0.0195****	**0.0040***	**-0.0002**	**0.0019***	**0.0394****	**0.0102+**
	(0.0077)	**(0.0064)**	**(0.0017)**	**(0.0011)**	**(0.0008)**	**(0.0124)**	**(0.0060)**
Joint Sig?	No	Yes	No	No	Yes	Yes	No
Obs	12629	77694	9844	9338	77694	10245	74022

Notes: Coefficients estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. The regression universe in panel a is restricted to men with employer-provided health in the year prior to the survey. The universe in panel b is restricted to men without employer-provided health in the year prior to the survey. Columns (1) - (5) are restricted to those who worked at least one week last year. Columns (6) - (7) are restricted to those currently employed. Regressions include age, age*veteran, state, year and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. "Joint Sig?" reports whether the veteran*post coefficients for the two populations are statistically significantly different from one another at the 5% level.

+ significant at 10%; * significant at 5%; ** significant at 1%